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THE

EDINBURGH NEW PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF THE

PROGRESSIVE DISCOVERIES AND IMPROVEMENTS

IN THE

SCIENCES AND THE ARTS.

CONDUCTED BY

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THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

Observations on the Nature, Longevity, and Size of Trees.

By ALEXANDER HARVEY, M.D., Lecturer on the Institutes of Medicine in the Marischal College and University of Aberdeen.* Communicated by the Author.

It is often asked, both in regard to trees generally, and also in respect of particular kinds of trees, *e. g.* the oak, the elm, the fir, &c., How long do they naturally live? And what is the size to which they naturally grow?

These questions manifestly involve the assumption, that every tree is a single or an individual object, in a sense precisely analogous to that in which an ox or a horse is so regarded; and they proceed on the principle that, however the appointed term of life and size of organism may vary in different kinds and species, all living beings, without exception, are subject to the laws of a limited duration of life, and of a definite size of organism.

This principle is unquestionably a sound one. No law of nature is more absolute or universal in its operation than the law of mortality. Every thing that lives, be it animal or vegetable, exists as such only for a certain time, on the expiry of which it passes into the state or condition of death. Nor is this left to be brought about by accidental causes. By these, indeed, it is often induced; but, independently of any such, the cessation of its vital actions, and the loss of its vital properties, is a fundamental law of the constitution of every living being. The provisions to which it owes its existence, and by which its vital actions are for a time performed, necessarily involve the extinction of its vital powers. Such changes are gradually wrought in it by the very agency of its vitality as are ultimately incompatible with the longer continuance of life, and death follows as a matter of course. And those changes are attended by, if they do not essentially consist in, a gradually increasing languor or sluggishness in the activity of the vital processes, and by a corresponding density and rigidity of the textures composing the organism—constituting a state to which the name of old age is given, which obtains uniformly when life is not prematurely cut short, and is indicative of the approach of death. Again, all organized beings have a definite size or bulk of organism. Of lifeless inorganic bodies, it cannot be affirmed that they possess any such quality, being smaller or larger to any conceivable extent, according as circumstances may determine. It is otherwise, however, with animal and vegetable organisms, which have naturally a fixed or standard size to which they grow, and from which they never greatly deviate. This fact in their history may not perhaps be so obviously true as that of a limited duration of life; nevertheless it can be shewn to be an equally general one, and the exceptions to it to be only apparent, not real.

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* Read before the Philosophical Society of Aberdeen, February 6, 1846.

But, acquiescing in the truth and universality of the principle now referred to, it may be confidently asserted in regard to trees, that, on the assumption stated as to their nature—to wit, that every individual tree is an individual plant—there are many facts in their history which it is difficult to reconcile with that principle; and, generally, that nothing definite or satisfactory has yet been ascertained respecting either the natural longevity or the natural size of any one species of tree,—a circumstance which contrasts remarkably with the precision of our knowledge, so far as it goes, as to these particulars in the case of animals, and the more, from the facilities that exist for making observations upon trees.

The assumption, however, is, I apprehend, a false one. It may be shewn, I think, in opposition to it, and it is the design of the following observations to prove, that a tree is actually a collection of distinct individual plants of the same species, the production of a series of successive years, and that of these plants each lives only one year, attains its full size within the year, and makes provision, in the form of buds, for the evolution of similar plants the following season :—the plants of each year shooting up in spring from buds formed by the plants of the previous year, and growing *parasitically* on the *persistent dead remains* of these ; acquiring their maturity in summer, and reaching to the height of a few inches only—seldom at least exceeding one or two feet ; passing into the state of old age and eventually dying in autumn—save only the buds which survive the winter ; and speedily after their death undergoing decomposition and disappearing, the dead stems and roots, however, remaining, to serve the purposes of a *temporary* soil, and of a *permanent mechanical support* to the plants of next year.

According to this view, a tree is nothing more than a congeries of annual and comparatively small-sized and slender plants, the propagation of which, from year to year in all time coming, is effectually provided for by buds; and the accumulation of which *en masse*, by the living growing as parasites on the residue of the dead, necessarily keeps pace with the annual succession of plants. And if this be the true account of the nature of trees, and of the mode of their formation, it will of course follow, that a tree is an individual precisely in the same sense as a body-corporate, or as a genealogical tree, and that,—contrary to the common opinion, but consistently with the principle before adverted to,—*there will be no limit,* except from purely accidental causes, *to the size it may attain, or the number of years it may live.*

The views thus briefly set forth appear to me to possess considerable interest in relation to the questions stated at the outset, inasmuch as, if well founded, they supply us with principles for the satisfactory solution of them, and thus serve at once to give precision to our ideas, and to relieve our curiosity in regard to the natural longevity and size of this class of objects. Moreover, they seem to me peculiarly interesting, as evincing the essential *unity* of the plans in this department of His works of the Author of Nature, and as affording unequivocal indications of *design*. For, if we can perceive the same general principle to pervade alike the economy of *tree-plants*, which *appear* to live for ages and to grow to an enormous size, and that of confessedly annual and obviously very small plants, which completely disappear at the close of every season, it cannot but enhance our conviction, that the whole vegetable kingdom, the extremes of it thus meeting, proceeds from one and the same Creator. And such a principle we may perceive; for both the one and the other are strictly annual and moderately sized plants, and are constructed exactly on the same general plan. Certain peculiarities there are, indeed, in the economy of tree-plants, viz., the property of growing as parasites on their fellows, and the persistency of their dead stems and roots,—peculiarities leading, in the course of years, when the species is greatly multiplied, to the formation of the masses known as trees. These peculiarities, however, in a physiological point of view, or in reference to merely physical causes, are entirely unessential. They are important only in relation to final causes, or as furnishing conclusions in Natural Theology. And with reference to these they are most important. For they clearly bespeak a design or purpose in the mind of the Creator: provisions they are of His, whereby, out of short and slender annuals, He forms *timber* for the use of man,—a substance which, if not indispensable to the existence, ministers at least in a thousand different ways to the comfort and wellbeing, of our race; but the production of which would be impossible, did not the economy of the plants in question thus differ from that of all other annuals.

The principles now advanced in regard to the nature of trees have no pretensions to originality. Though not suggested by any knowledge or recollection of the circumstance, they are, in fact, the principles long ago put forth by M. Du Petit Thouars, respecting the nature of buds. The application of them, however, here made to the questions already specified, does not appear to have entered into the view of that ingenious physiologist. So far, at least, as I can gather from the writings of Richard, Lindley, and others, M. Thouars seems to have applied them only in explanation of the nature and the mode of formation of the woody layer annually produced in exogenous trees. At all events, both the principles themselves, and the application I have made of them, manifestly differ from the views commonly entertained as to the nature and the natural longevity and size of trees. Nor can I find anything amounting to a distinct recognition or detailed exposition of them in any of our systematic works on Botany. If they are correct, however, their scientific interest and importance clearly demand this, even in the merest outline that can be given of vegetable physiology. And as the evidence in support of them appears to me conclusive, I am inclined to hope that the present attempt to bring them prominently forward may not be deemed undeserving of attention.*

In proceeding to vindicate, and more fully to illustrate, those principles, it is desirable, in the first instance, to inquire what is known in regard to the natural longevity, and the natural size of trees, according to the view usually taken of the nature of this class of objects. The inquiry, by shewing how imperfect that knowledge is, and how difficult it is to impart to it any character of precision, will naturally pave the way to the consideration of the principles in question, and probably dispose to a more cordial reception of them.

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* Since these observations were put together, I have seen a foot-note in Roget's Bridgewater Treatise (vol. ii., pp. 555–6, 3d Ed.), wherein the views promulgated in the text are briefly hinted at, on the high authority of De Candole. More recently, I have had the satisfaction of finding a clear and distinct statement of them in Dr Carpenter's *Manual* of Physiology. "A forest tree may go on extending itself to an almost indefinite extent;" "but the increase is produced, not so much by the continued development of the individual, as by the continued production of new individuals which remain in connection with the original. Thus, each bud of a tree may be regarded as a distinct individual; because, if placed under favourable circumstances, it can maintain its life by itself, and can perform all the actions proper to the species."—Pp. 2, 3.

no idea whatever of the natural or allotted duration and size of trees. They do not even inform us as to the extreme limits to which their lives may be protracted, or the extreme height and thickness to which they may grow. For it may be confidently asserted, that, in different parts of the world, there are individuals of almost all kinds of trees which have already stood as many years, and grown to as great a size as any of the species have ever been known to do, and many of which are still vigorous and growing, and, what is remarkable, exhibit as yet no signs of what can properly be regarded as old age. Much of their trunk may be hollowed out from decay of the heart-wood, and many of their larger branches may have been destroyed in the lapse of time; but the great body, or a large part of many of them, remains, and evinces as great activity in the vital processes as ever, *i. e.*, is the seat of as vigorous a circulation of sap, and forms and puts forth leaves and flowers, and fruit, as large and perfect as in its earliest years, and is every year having additional bulk given to it.

The considerations now stated, and especially the facts relative to the Banian, may very naturally suggest a doubt, whether, with respect to their longevity, there is not some peculiarity in trees beyond a merely very prolonged existence; nay, though (according to the common opinion as to their nature) it seems absurd seriously to entertain the idea, whether there is not in their case, so far as yet appears, an actual exemption from the law of mortality. Some such idea, at least, may not unreasonably be supposed to have been in the mind of Richard, when he remarked of the cedars of Lebanon, that they appear to be indestructible,—a remark which is still more applicable perhaps to the Banian, but which, if it has any meaning, is equivalent to saying, that they appear to live for ever, and obviously involves the assumption that the law of mortality is not universally operative.

Such is the present unsatisfactory state of our knowledge with respect to the longevity and the size of trees, regard being had to the popular notion as to their nature. In the case of each species of animal, the natural term of life, and the appointed size of organism, are either known to us, or may, without difficulty, be ascertained by us. With regard to trees, however, of whatever species, these points in their history may be said to be absolutely unknown to us, and that, too, as before observed, notwithstanding the facilities which exist for making observations upon them. Every such object is looked upon as a single individual, in the same sense that a man or a dog is so regarded; and while it is believed to be subject to the law of mortality, and to the law of a definite size of organism, it is believed also to be, as compared with any known animal, very long-lived, and capable of attaining to a gigantic bulk. But no more precise idea than this is entertained as to its longevity or its size, and even this view of the matter is beset with considerations of perplexity.

II. But if the principles formerly advanced in regard to the nature of trees are well founded, that perplexity will be obviated, and an accurate idea may be formed as to the longevity and the size of this class of objects.

Agreeably to those principles, a tree is not what it is usually regarded, nor what it appears to be, a single or an individual *plant*, capable as such of living for many years or ages, and of attaining to an enormous size. On the contrary, it is a collection, congeries, or congregation of individual plants of the same species, the production of a series of successive years, and consists, at mid-summer, partly of living and entire plants, the produce of the existing year, and partly and chiefly of the persistent dead remains of the plants of bygone years. And of the individuals composing it, each lives only one year, reaches its full size within the year, and on dying at the close of it completely disappears, save only the buds which survive the winter, and the dead stems and roots which are to serve the purposes both of a temporary soil, and of a permanent mechanical support to the plants of next year. And, accordingly, the production of the aggregate of dead and living plants is referable to the living plants of each year growing parasitically at the extremities of, and also either around (as in Exogens) or within (as in Endogens) the dead stems and roots of the plants of the previous year.

Those principles, however, it may be remarked, are only a part of a proposition in vegetable physiology of a still more general character; and it may conduce to a clearer apprehension of them, to bring that proposition formally into view. It may be thus stated:—That all plants, without exception, even those called perennial, are strictly *annual* productions, live therefore only one year, and reach their full dimensions within the year; that is to say, that all plants spring up anew each year, either from seeds or buds, and attain their maturity within the year,—forming in the course of it either seeds or buds (or both) for the development of similar plants the following year; that as the season advances, their vital actions languish, and a change in the matter of their organism takes place, both constituting their old age; that at the close of the season they die; that then the structures composing them speedily undergo either an entire or a partial disintegration; in the one case, wholly disappearing, in the other, some portion remaining to serve ulterior purposes in the vegetable economy of nature, but still remaining only as dead vegetable matter. And, in connection with, and as forming part of this general proposition, that the only difference between the plants called annual, and those called perennial is, that while the former produce seeds only for the propagation of the species, and are reared annually from seeds alone, the latter produce both seeds and buds, and *qua* perennial, spring up each year from buds; and, therefore, that seeds and buds are potentially of the same nature; the only difference between them, and that not a uniform one, being, that seeds are free and detached, buds fixed and adherent.*

If, now, the question be formally proposed in regard to any given tree, How long does it naturally live ? the answer must be—contrary, however, to the common opinion—that there is no limit to the age it may attain, or the number of years it may live, except what is imposed by purely *accidental* causes; because, according to the principles insisted on as to the nature of trees, there is no natural limit to the annual propagation from buds of the individual plants of which every such object is truly composed. According to this view, the observation of Richard formerly quoted, to wit, that the cedars of Lebanon appear to be indestructible, is perfectly intelligible, involves no violation of the principle that all living beings are subject to the law and the dominion of death, and is applicable besides to all trees. And if it be asked in respect of any given tree, what is the size to which it naturally grows ? the proper answer is—contrary, again, to the popular belief—that there is no natural limit thereto, and no actual limit, except from such accidental causes as prevent the formation of buds, or the evolution of new plants therefrom.

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* M. Du Petit Thouars; see Richard, Op. cit., p. 103.—"Les bourgeons donnent naissance à des *scions* ou jeunes branches chargées de feuilles, et le plus souvent de fleurs. Chaque bourgeon a une existence en quelque sorte independante de celle des autres. M. Du Petit Thouars les regarde comme analogues, dans leur developpement et leur structure, aux embryons renfermés dans l'interieur des graines, qui, par l'acte de germination, developpent une jeune tige que l'on peut comparer, avec juste raison, au scion produit par l'evolution d'un bourgeon. Aussi donne-t-il à ces derniers le nom d'*embryons fixés* ou adherens, par opposition à celui d'*embryons libres*, conservé pour ceux renfermés dans l'interieur de la graine."

If, however, the like questions be put in respect, not of individual *trees*, but of individual *tree-plants*—of the oak, the elm, the fir, for example, viewed simply as plants, and independently of their parasitic relations to others of their respective species, very different answers must be returned. The answer to the former question will be, that they live, one and all of them, only for a single year, and are, as regards their longevity, on the same footing with confessedly annual plants. And in answer to the latter, it may suffice to state, that, as they all attain their maturity within the year, so the size of any of them may be accurately judged of by observation of the seedling plants of its kind growing in the forester's nursery, or of the yearly shoots issuing from the buds on a tree of that particular species; and that while subject to some variety, it does not, in general, in any species, exceed a few inches, or, at the utmost, a very few feet.

But if the representation which has been made of their nature is well founded, how comes it, it may be asked, that we nowhere find, what we might expect to see, trees evincing by their appearance that they are probably coeval with the creation of the world—coeval at least with the deluge, or the age immediately succeeding that catastrophe? Assuming that none such exist, it is, I apprehend, a sufficient answer to that question, to say, that no tree then existing has been able to withstand the "manifold changes and chances" of time. All dead organic matter is subject to, and sooner or later undergoes, chemical change and disintegration; and by reason thereof the heart-wood of every tree disappears after a time, leaving the trunk hollow within, often reducing it to a mere shell, and thus necessarily weakening the mechanical support given by it to the superincumbent mass. Again, after a tree has stood for many years, the vast height and breadth of surface presented to the wind will enable this to act on it to its destruction at an advantage infinitely greater than in its earlier years. And it is obvious to remark, that the older a tree becomes, its liability to be uprooted by any passing storm of wind increases in a double ratio; on the one hand, from its greater size, and on the other, from the more extensive decay and removal of the heart-wood. Add to these, the exhaustion of nourishment in the soil, which may, and often does occur, and the inevitable occurrence, in the course of ages, of a thousand other destructive influences—of frost, fire, lightning, hurricanes, the necessities and the caprices of man himself,—and a calculation of chances puts it beyond all doubt, that every tree, or almost every tree, then existing, must long ere now have disappeared from off the face of the earth.

It remains to substantiate the allegations that have been made in regard to the *nature* of trees. If these can be established, the inferences as to their natural longevity, and their natural size, must necessarily be true.

Now, the evidence to be adduced will consist in shewing, *first*, That the annual growths proceeding from the buds constitute, severally, perfect and independent plants; and that a succession of such plants may be kept up from year to year, for an indefinite period, from buds alone; and, *secondly*, That at the end of the year, the annual plants or growths in question, with the exception, of course, of the newly formed buds, cease to be, and never afterwards become, the seat of any vital action.

I. First, then, the annual growths emanating from the buds constitute, severally, perfect and independent plants; and a succession of such plants may be kept up from year to year, for ever, from buds alone.

On the return of spring, "when the earth, by an annual miracle, rises again, as from her grave, into life and beauty," we see the buds on each of the stems of the previous year, first swelling, and afterwards sending out an entirely new stem, new leaves, flowers, and buds, all of which are pervaded and connected together by a new set of circulating vessels or cells. And in the course of the season seed forms and is matured. Now, in this annual formation, we have issuing from the buds on the stems of last year all the parts essential to the constitution of a perfect plant; and on the new stem of this year we have buds provided for the evolution of such a plant next year.

With the view of shewing more distinctly, at once the individuality and the independency of the growths thus formed, and their claim to be regarded as perfect plants, and the ability of each of them to reproduce its kind annually in endless perpetuity from buds, reference may, in the first instance, be made to what obtains in the potato-plant—a plant which, though not a tree, contains all the elements of one, and is equally perennial in its duration as any tree. The tuber familiarly known as the potato bears an exact resemblance to, and is essentially of the same nature with, the yearly shoot or stem of a tree. It is, in fact, an underground stem, consisting of a layer of bark, and a layer of woody tissue, enclosing a mass of pith, and furnished with buds. This underground stem, when planted in spring, sends out from each of its buds a growth which has a stem (underground), and leaves and flowers, and forms buds and seed, structures which are exact counterparts of those composing the growths issuing from the buds of trees. Does any one doubt that the annual potato-growths constitute perfect and independent plants? Does any one doubt that from the buds alone, without ever having recourse to seed, a succession of such potato-plants may be kept up from year to year for ever? I apprehend not. If so, we are warranted in ascribing the same character to the annual growths of tree-plants emanating from their buds, and in inferring that the succession of them from year to year, as congregated together and constituting a tree, may equally go on for ever.

The only difference, in fact, between a tree-plant and the potato-plant, lies in the situation of their respective stems, in the changes which these and their roots respectively undergo after losing their vitality, and in the habitudes of their respective offspring; the stems of the one being above, those of the other under ground; the dead stems and roots of the one continuing undecomposed and persistent for years or ages, those of the other decaying early the following year, when planted or left in the ground, and passing away; the offspring of the one growing together, and as parasites, on the persistent dead remains of their parent, those of the other striking down singly and separately into the soil, and having no connection with any portion of their parents, the residue of which has, in fact, ere now wholly disappeared, and nothing remaining around which, as a common centre, and a mechanical support, they could grow as parasites. Had it suited the purposes, immediate and remote, which the Author of Nature had in view in giving them existence, we might have had the respective peculiarities in the economy of the plants completely reversed,—the potato-plant and its progeny growing together and parasitically, and forming by their aggregation a true potato-tree, fantastic, doubtless, in its aspect, but possessing, as a whole, the same individuality which common opinion ascribes to an ordinary tree, and the separate plants being regarded and spoken of merely as annual growths; the tree-plants, on the contrary, growing and extending themselves year by year as distinct and separate individuals, and the so-called annual growths being looked upon as perfect and independent plants.

Reverting, now, to what takes place in trees, the processes of grafting, of budding, and of slipping, seem to me to furnish sufficient evidence that the allegations made as to the annual growths are well-founded, and indeed to be explicable only by a reference to the principles involved in them. From the yearly bud taken from one kind of tree and duly grafted on another, of the same natural family, though of a different species, we obtain the following year a growth, and in the course of years a tree, exactly similar to the tree, and to the other growths of the tree, whence it was derived; the tree thus formed, though growing on another, preserving, nevertheless, its own distinctive character, having its own peculiar leaves and blossom, producing its own peculiar fruit, and being in every way as perfect a tree as if it had been raised from a seed, and had grown up independently from the ground. And it is not unimportant to observe, as instanced in our various "fruit" trees, that any particular variety may be, in this way, not only multiplied indefinitely, but preserved *in perpetuity*, although the original, or any single tree of that variety, will not (from accidental causes, however) continue to last for ever. Similar observations apply to the indefinite multiplication and endless perpetuation of such trees (*e.g.* the willow) as admit of being artificially propagated by slips or layers. And it is thus that the Banian tree extends itself naturally, dropping branches provided with buds, fixing themselves in the soil, and becoming vast trunks, and these readily convertible (I presume) into separate and perfectly independent trees, by artificially severing their connections above. And the trees thus produced admit of a similar extension, and the trunks proceeding from them of a similar conversion.

The evidence adduced under this general head seems to me complete and decisive. It may be asked, however, where are the *roots* of the annual growths (and alleged perfect plants) in trees? And, again, what is there in avowedly annual and perfect plants, or even in such perennial plants as the potato, analogous to the *woody layer* in exogenous trees, which extends downwards from the base of the shoots into the soil?

These questions may easily be answered. With reference to the former, it may be remarked, that the circumstance of the growths in question being destitute of true roots, supposing this to be the case, would by no means take from them the character of entire and perfect plants, if it could be shewn that the office of roots is otherwise adequately provided for. And, with reference to the latter, it may be observed, that the fact of a structure entering into their constitution, not existing in other plants, would not at all affect their claim to be so regarded, particularly if it could be shewn that that structure is required to meet some condition of their existence peculiar to themselves, or to serve some ulterior purpose in the economy of nature. And the woody layer, it is to be remembered, is strictly an annual formation, and so far accords with the view taken of the growths in question as being annual plants.

Now, the structure referred to—the woody layer—clearly subserves, immediately, the purpose of a mechanical support to the growths or plants of the same year's formation with itself, and remotely that of producing timber. Without it trees could scarcely grow at all, or, if they could, would be of little use to man. But it serves also the office of roots to those growths or plants, being the channel by which the nutritive matters in the soil are conveyed upwards to the growing stems, and leaves, and flowers. And if it thus serves these various purposes, the questions stated must be regarded as satisfactorily disposed of. The only question will be, whether the woody layer, in its origin and mode of formation, be actually of the nature of, or rather identical with, roots, and only secondarily intended for a mechanical support, and for the production of timber; or whether it is truly a special formation for the accomplishment of these latter objects, and only virtually of the nature of roots?

After what has been stated, however, this other question is of no real practical importance in relation to our present inquiry. The woody layer may be formed in the manner that M. Du Petit Thouars supposes, or in that insisted on by M. Mirbel and others. If in the former, it constitutes true roots, and is nothing more than "a mass of roots;"* if in the latter, it is only virtually roots. The mode of its formation, however, and its precise character, is unimportant. Still, if it is genuine roots, as M. Thouars maintains, and as Dr Lindley and Dr Carpenter agree with him in believing,* it will at once follow that the growths in question have roots, and that having these, they have no extraneous element entering into their composition. And a positive and very valuable fact will be added to the general body of evidence already adduced in support of the view, that those growths possess the character ascribed to them.

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* Dr Lindley, *Introduction to Botany*, First Edition, p. 245.

II. Secondly, at the close of every year, the annual growths or plants, with the exception, of course, of the newly-formed buds, cease to be, and never afterwards become, the seat of any vital action, *i. e.*, they die, and never afterwards live.

This is sufficiently obvious as regards the leaves and flowers, which wither, fall off, and completely disappear. It is equally true, however, of what remains of the other parts of the plants, *i. e.*, of the roots and the woody stems or shoots.

But on what grounds are we entitled to say that these parts then die, and never again live?

1. In the first place, because after the fall of the leaves, and during subsequent years, no growth or increase of the organic matter composing them takes place, as should be exhibited in an increase of their length and thickness, and produced in the way that the leaflet of spring is gradually developed into the full grown leaf of summer. They appear, indeed, to elongate and become thicker, *i. e.*, to grow in length and breadth. This growth, however, is not a real extension of the parts in question, as it is in the leaflet; it is a new and independent formation at their extremities, and either around or within them, and may, by examination, be seen to

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* "The most consistent account of its development is that given by Du Petit Thouars, who, followed by Lindley, regards the fibrous [woody] tissue as formed in the leaves, and growing downwards into the cambium, just as roots are prolonged into the soil. This view would liken the woody fibres to the roots of the buds; and such a comparison, though at first sight improbable, is fully borne out by facts."—Carpenter, *Principles of General and Comparative Physiology*, 1st Ed., p. 278.

be quite distinct from them,* being, in fact, the roots and stems of the new plants attached to and growing upon them.

2. In the second place, no removal, by interstitial absorption, of their substance, and replacement of this by new vegetable tissue, ever takes place. The tissue composing them undergoes no subsequent change of this kind. Once formed, it is never afterwards the seat of any change corresponding to the renewal of substance, which is continually going on in the living tissues of animals.†

And, with reference to this, it may be remarked, that the absence of any such change goes far to shew that, on their growth being completed, the parts in question are really dead. Judging from what obtains in animals, many of which truly live for years, it is not unreasonable to infer that a continual or frequent change of substance is essential to the maintenance of the vitality of any structure which really continues for any length of time to be the seat of vital action. The brain, for example, of an animal is possessed of vitality, and performs important vital actions during the whole time that the animal lives ; but the maintenance of its vitality, and the performance of its vital actions, appear to be dependent on, and to involve, a continual change in the substance of the organ. Moreover, the rapidity of that change seems to be exactly proportioned to, and to afford a measure of, the frequency and energy wherewith the vital actions of the organ are carried on,—to be more rapid when these are often and actively performed, and less rapid when they are seldom and feebly exerted. Is it, then, an unfair inference, that the absence of that change, and the inability to undergo it in any tissue, are tantamount to this tissue being the seat of no vital action, or destitute of vitality, after its formation is completed?

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* Lindley, Op. cit., p. 228 and p. 241, *et seq.*

† "The economy of vegetables is fitted for their office of constantly converting inorganic into organized matter, by this peculiarity, that their nutrition is maintained without any such function as the interstitial absorption of animals; and necessarily involves, during the whole time that any living actions are going on, continual additions to their substance."—Alison, *Outlines of Physiology*, 3d Ed., p. 12.

"In vegetables there is none of that absorption of the different parts which takes place in animals. The matter of which they are composed, being once deposited, is never taken up again ; whilst in animals there is a constant process going on, by which the old matter is taken away and the new deposited, and the organs thus renewed."—Dr Ware, in Smellie's *Phil. of Nat. Hist.*, Introduction, chap. ii.

3. In the third place, we know, that after a time the heart-wood decays and disappears, and that this change may go to such an extent as to destroy a large part of the entire thickness of the trunk, without, however, in the least impairing the vegetation going on at the extremities, and on the exterior of the tree. This it is easy to understand, according to the view here taken of the nature and duration of life in trees; but very difficult on the supposition of an entire tree constituting a single or an individual plant, and being endowed with vitality in its every part. On this supposition, such a change occurring in the heart-wood should spread to the adjoining living tissues, and sooner or later, but before long, destroy the vitality of the whole fabric. This, however, does not happen; nor is the complete and premature decay of an entire tree ever to be ascribed to the agency of such a cause.

It may be supposed, however, that although the old stems and roots, after the year of their formation, are the seat of no nutritive organic change, and actually decay and disappear in the course of years, the circulation of the sap moving through them the following and during several subsequent years, is a clear proof that they retain their vitality for a much longer period than is here allowed.

It does not therefore follow, however, that the parts in question are alive. To warrant such an inference, it must be shewn that they contribute *actually* and *actively* towards the movement, and that, too, in a way not referable merely to their porosity, or to any other simply physical property which they may possess. The experiment is well known of strewing cress or mustard seeds on a vessel covered with flannel, placing that in a saucer filled and regularly supplied with water, and of finding the seeds vegetating and covering the vessel with living plants—the flannel, by reason of its porosity, conveying the water upwards from the saucer to the living seeds and plants. No one, however, would say that the flannel is alive. No more are we entitled to affirm, that the old stems and roots of a tree are alive, because of the sap moving through them to the growing parts above. They may be merely the medium or channel of its transmission, and aid in effecting this in the same way that the flannel does.

Unquestionably, the movement of the sap is a vital action, and due to vital agency. This agency, however, has its seat in the living buds, and in the living structures proceeding from them, and actually growing. It is directly connected with, and dependent on, the vital processes going on there during the spring and summer.* The first movement of the sap in spring is in the immediate vicinity of the buds. The fluid there, previously at rest, is the first to be set in motion, and its movement is determined by the act of vegetation beginning in the buds under the influence of heat and light. The subsequent increase in the activity of that process demanding additional and greater supplies of sap, an agency is exerted which operates downwards in the direction of the soil, and causes the nourishing fluid to ascend. And it is farther important to remark, that the movement of sap from the soil upwards through the trunk to the parts where vital actions are undoubtedly going on is, the whole season through, regulated by the activity of these actions. Of all this we have several decisive proofs. If a branch of a tree, standing in the open air, be introduced into a hot-house at a time when no vegetation, and no circulation of sap is going on in the tree, the buds of that branch will vegetate, and sap will circulate through it, while as yet nothing of the sort is in progress in any of the other branches of the tree.† It is quite inconceivable that the roots and stems should exert so exclusive an agency, or have any share in producing so partial a change. Again, if the buds be cut off from a branch prior to the commencement of the annual process of vegetation, no sap will pass into the branch during the entire spring and summer, although the other branches, not thus mutilated, will be filled with it. Once more, if, at a later period in the season, the leaves be stripped from off a branch, the flow of sap through it will speedily, if not immediately, cease.

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* "It is evident, then, that the force, whatever be its nature, by which the continued movement is kept up, must be developed by the processes to which that movement is subservient; in other words, that the changes involved in the acts of nutrition and secretion, are the real source of the motor power."—Carpenter, *Manual of Physiology*, p. 315.

† Alison, *Outlines of Physiology*, p. 70; Carpenter, *Manual of Physiology*, p. 313.

What may be the nature of the agency thus exerted in the growing buds and leaves which causes the sap to circulate, and regulates the quantity of it passing through the old stems and roots, it is not easy to say, and for our present purpose unnecessary to inquire. But the old stems and roots may be no farther concerned in it, than as being the channels through which the nourishing fluid passes upwards from the soil. And no facts yet known to physiologists demonstrate that they have any other share in it.

But, in the course of the season, there is a *descending*, as well as an upward, movement of the sap. And the former must be regarded as being equally of a vital nature as the latter, and equally due to vital agency. Does not that movement, at least, argue vitality and vital action in the old stems and roots? I apprehend not; and for this reason, that while the ascending current seems referable to the processes going on in the buds and leaves, the movement in question appears to be connected with the formation of the woody layer all over the exterior of the tree, and referable to the process by which that structure is evolved. Whether the woody layer be of the nature supposed by M. Thouars, or of that insisted by other physiologists, is immaterial. It is distinct from the woody layer of previous years, and is of the same year's formation with the existing leaves and flowers. And it requires for its organization equally as these do for theirs, a supply of prepared or elaborated sap. But the sap is elaborated only in the leaves; and as the woody layer extends from the base of these *downwards* to the extremities of, and even beyond, the roots of last year, so that sap can only be supplied from *above*, and must *descend*, in order to the formation of the tissue in question. This descent, however, may be solely connected with that formation, and there is no proof that it has any thing to do, directly, at least, with changes going on in any of the other structures of the tree.

It has thus, it is hoped, been satisfactorily made out, *first*, that the growths emanating from the buds of trees constitute perfect and independent plants; and, *secondly*, that what remains of them, after the fall of the leaves and flowers, and fruit in autumn, with the single exception of the new buds, ceases to be, and never afterwards becomes, the seat of any vital action.

And if this be conceded, it will probably be allowed also, that the view which has here been taken of the nature and of the natural longevity and size of trees is well-founded; that is to say, that a tree is simply a collection of annual plants of the same species, the production of a series of successive years,—the individual plants of each year shooting up in spring from buds adherent to the persistent dead remains of the plants of the previous year, growing as *parasites* on these remains, putting on the characters of *old age* in autumn, and speedily thereafter dying, having made provision, however, in summer, in the form of buds, for the reproduction of similar plants the following year. And that being thus evolved, and thus growing from year to year, and having no *natural* limit to their increase and aggregation, there is no natural limit to the age or to the size to which the tree collectively formed by them may reach.

The statement repeatedly made, that the persistent dead remains of the plants of the previous year serve as a *mechanical support* to the plants of the following year, does not appear to require any explanation. With regard to the other statement, that they serve as a *temporary soil* to these plants, it may be observed, that the buds are always placed in intimate connection with the *pith* or *medulla* of the shoots to which they are adherent, and that the pith is soft and juicy in spring, but in the course of the season becomes dry and shrivelled. According to M. Thouars, the buds vegetate in the first instance at the expense of the pith, deriving from it the materials of their development in spring. When this supply of nourishment is exhausted, or at least when the buds send out their own proper roots, *i. e.*, the fibres formerly mentioned, which, by their interlacement form the woody layer, another supply is provided in the succulent *cambium* into which those fibres pass, and along which they descend to the soil. From this, of course, is ultimately derived the materials, or a part of the materials, necessary for the growth of the young plants emanating from the buds.

Some Remarks on the High Temperature in the United Mines in Cornwall.[*] By Robert Were Fox. Communicated by the Author.

The temperature of some of the deeper parts of the United Mines[†] has long been observed to be remarkably high; and it has greatly increased with the increasing depth of the excavations.

Captain Youren, one of the agents of the mine, informs me, that near the eastern extremity of the deepest level, on the "*middle lode*," there is a spring or jet of water, discharging about 94 gallons a minute, at the temperature of 106⅓°[‡] Fahr. This level is 250 fathoms below the surface, and about 200 fathoms under the level of the sea. The "lode" has an underlie or dip of about 2½ feet in a fathom towards the north, and the water flows from its *northern* or *upper wall;* whilst, from the opposite side, or *southern wall* of the lode, at the distance of only 3½ feet, there is another spring, discharging 30 gallons of water in a minute, at the temperature of 97¾° Fahrenheit. The air near both these springs was found to be at 104¼° Fahrenheit; and "*killas*" is the only rock which has been seen within 30 fathoms of them. Granite occurs at a considerable distance westward of the place, and two "elvan courses" traverse the mine in nearly the same E. and W. direction as the lode.

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[*] Read 3d September 1846, before the Royal Cornwall Polytechnic Society.

[†] This mine (for it is one concern) continues to produce abundance of copper ore. It is situated in the parish of Gwennap, about eight miles to the northward, or almost NNW. of Falmouth, and is several miles from the sea.

[‡] The thermometer employed has been carefully compared with a standard one, and found to be ¼ of a degree too high, so that this small amount must be deducted from the results, making them 106·08°, 97·5°, and 104°, respectively.

I have found that ¼ of a pint of the water from the warmer spring contained 15 grains of saline matter, consisting of muriate of lime and common salt—in about equal proportions,—with a trace of sulphuric acid, probably combined with lime. In the same quantity of the cooler water, only 10½ grains of muriate of lime and common salt were found,—the latter in less proportion than the former,—and in this water also there was a slight trace of sulphuric acid. In both instances the water was clear, saline to the taste, and without any metallic salt.

It may, I think, be inferred from the saline contents of these springs, that they have a common origin or source; whilst their high temperatures indicate their having come from a considerable depth, and the quantity of water they discharge, that the lode, or rocks beneath, must be very pervious to it. In these instances, at least, there are no grounds for supposing that any chemical decomposition of the sulphur ores in the lode has caused the high temperature of the water, or contributed to it in any degree, seeing that it contains no metallic, and scarcely any sulphate salt.

The difference in the heat of the two springs may, perhaps, in part, be attributed to the tendency of the warmer currents to rise towards the upper wall of the lode; and, still more, to that of water at a much lower temperature passing from superior strata down upon the inclined surface of the lower wall, where, mixing with the water rising from below, the temperature becomes modified, as well as the proportion of the saline contents.

It cannot be doubted that ascending and descending currents of water, more or less copious, and at different degrees of temperature, abound in the veins and fissures of the earth, and often at the junctions of different rocks, and that they must have a great influence in modifying the subterranean temperature, and in different degrees in different places.

Common salt is of rare occurrence in our mines; its presence in the water in question cannot well be attributed to the flowing of sea-water into the excavation, in consequence of its local or direct pressure; for if some miles of distance from the coasts did not render this highly improbable, the considerable streams at very high temperatures, and very constant too (as appears from observations made at different times), are facts not consistent with such an explanation. If the subterranean jets of water were caused by the inroads of a neighbouring sea, we should expect to find them at comparatively low temperatures, and these diminishing in proportion to the duration and amount of the influx.

The salt may, however, have been derived from the ocean, in consequence of the latter penetrating into the earth at its greater depths, or even at its lesser ones, which, under different given circumstances, it may be supposed to do. In either case, the salt water would, from its superior specific gravity, have a tendency to descend through the heated and less saline water in the veins, fissures, &c., where the fluids becoming gradually more or less mixed and extended in different directions, might ultimately appear in some of our mines, brought up, perhaps, in the largest proportions, by the upward tendency of the more heated currents of water.

Further Evidence of the Existence of Glaciers in Scotland in Ancient Times. By Charles Maclaren, Esq., F.R.S.E. Communicated by the Author.

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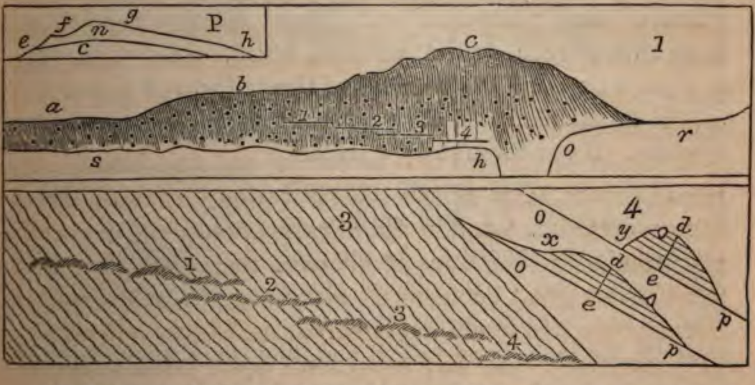
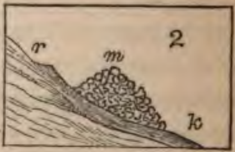


In October 1845, I published in the *Scotsman* an account of certain phenomena at Gareloch, which seemed to me to prove very clearly that a glacier formerly occupied the bottom and sides of the valley in which that loch lies. The more important of these phenomena are, first, scratches and groovings on the rocks, exactly such as glaciers produce on the bottom and sides of the valleys containing them, and which no other natural agent is known to produce, unless it be the iceberg, which, in many cases, is merely a detached portion of a glacier set afloat; second, large fragments of rocks scattered over the surface, such as glaciers which have receded or disappeared leave behind them; third, a bank of gravel and clay crossing the loch at its lower end, and resembling the *terminal moraine* found at the foot of a glacier. The paper was afterwards honoured with a place in the Edinburgh Philosophical Journal. My observations were made in the summer, and about the same time Professor Forbes obtained evidence of the former existence of glaciers in the Isle of Skye. A second visit to Gareloch, in July last, has afforded a variety of additional facts which go to support the conclusion previously come to —namely, *that Scotland at an ancient epoch had an arctic climate similar probably to that of Greenland or Labrador.* I shall arrange my recent observations under a few heads.

Lateral Moraines.

"Moraines" are accompaniments of glaciers, composed of stony matter, and afford good evidence of their former existence after they have disappeared. They are of two kinds, "lateral" and "terminal." A lateral moraine consists of fragments of rock, gravel, and soil, which, loosened by rain or frost from the elevated sides of the valley, roll down, and collect on the flanks of the glacier, in long lines resembling terraces. If cut through by a stream, the profile or cross section presents this appearance *r k* the surface of the rock, *m* the moraine: As the glacier bears a part of the debris along with it, the moraine increases in size towards the foot of the valley, and, like the glacier, it is not horizontal in the longitudinal direction, but less or more inclined. This inclined position distinguishes moraines from



raised beaches. At the lower end of the glacier the debris, joined by other fragmentary matter borne on its central parts, and by fine mud escaping from its bottom, form a dike or angular mound, extending partly or wholly across the valley, which is then called a "terminal moraine." The terminal is a continuation of the lateral moraine, but changed in direction.

In my paper of 1st October 1845, I expressed an opinion that the bank of gravel which extends across the loch at Row had been originally a terminal moraine. I was disappointed, however, to find that the district near the head of the loch presented no distinct indications of "lateral moraines." My station this year being three miles farther south, I had an opportunity of examining the hills towards the foot of the loch, and had the satisfaction to find portions of what I considered ancient lateral moraines.

Fig. 1 is a miniature view of the hills on the east side of Gareloch, as they appear to an eye placed on the hills of the opposite side.

a, The northern portion, is a saddle-shaped ridge, about 600 feet high, presenting a straight and remarkably uniform outline for a length of more than a mile.

b, Another portion, about one mile long, saddle-shaped, with a very uniform outline, and about 900 feet high.

c, The southern summit, about 1250 feet high, with a waving outline, and of a roundish shape.

shor, The line of the shore, which runs out in a projecting point at Row (*ho*), the form of which will be seen at *i*, figure 5.

1, 2, 3, 4, indicate the position of certain terraces, or rather remnants of terraces, which seem to me to have the character of lateral moraines.

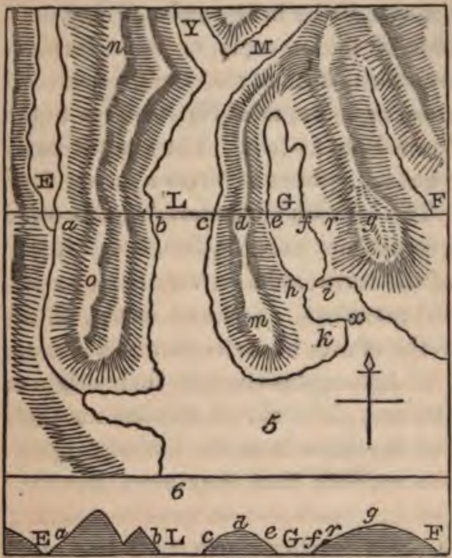
Fig. 3 is an enlarged view of these terraces, which will give a better idea of their form. About two miles above Row Point, a footpath commencing at the Free Church leads up the hill from the loch side. At an elevation of 500 feet (vertical) a sort of mound or terrace is met with, running south and north along the side of the hill. The numerous streams which descend from the higher parts of the hill have breached it at many places, and aided probably by gliding masses of snow (avalanches), have swept parts of it away. Instead of a continued terrace, therefore, it presents a succession of little knolls or protuberances, sometimes in contact at the bottom, sometimes separated by wide gaps, but in general running nearly in one line. That line is not horizontal, but inclined to the foot of the loch at a low angle, varying from two degrees to ten. There are three, or perhaps four, of these terraces. In some parts they are obscure and equivocal, but in others very distinct. The higher ones generally overlap the lower, so that two terraces, the one a hundred or two hundred feet below the other, are found at some parts, for the space of a furlong. Sometimes the upper seems to unite with and merge into the lower. Nos. 1 and 2 present this appearance. But they succeed one another, much in the manner indicated in the diagram. The highest, 1, is about 550 feet of vertical height above the loch at its north end, and may be traced for about a mile. The lowest, 4, is exceedingly well marked above the villa of Brownfield, where it forms a terrace nearly level, and more than 100 feet in breadth. It descends pretty rapidly, and at Leruel is about 60 feet above the loch. Its course southward is interrupted by a ravine behind Altdonach, beyond which it reappears, and finally merges into the projecting point of Row, with an elevation of 40 feet, much in the same manner apparently as the lateral moraine merges into the terminal in a Swiss glacier.

The openings cut through the terraces exhibit their composition. They consist chiefly of clay, mixed with fine chips of mica-slate, along with gravel, and with blocks of various sizes. Some of the latter are of great magnitude, 5, 8, or 10 cubic yards. These are seldom much rounded, and nineteen out of twenty are of the rock of the district. Some are buried in the soil, and only discovered in the sections made by the torrents ; others protrude through the turf. The proportion of soft matter, clay or sand, was larger than I expected to find, for Agassiz's plates represent the lateral moraines of existing glaciers as chiefly formed of rocky fragments. But on referring to his book, the idea his description conveys is not inconsistent with the facts I observed. He says—" In moraines, properly so called, that is to say, upon the sides (bords) of glaciers, we meet with blocks of all dimensions pell-mell, some completely angular, others more or less rounded, with pebbles of every size, and even sand of extreme fineness, or in the state of finely triturated clay. *Schists*, limestones, and particularly marls, are of all rocks those which disintegrate the most rapidly. These last, instead of being transformed into rounded pebbles, become a soft paste, forming a bed of mud under the glacier, or dikes of clay on its sides." (*Etudes sur les Glaciers*, p. 101.) There are no marls here, but the schists, which are partly micaceous, partly chloritic, probably disintegrate with considerable ease, and much of what were fragments at first may now be in the state of clay.

Fig. 4 shews the profile of the terraces, or their form in a cross section; *o p* the natural declivity of the ground; *e d* the thickness of the terrace, or its depth at right angles to the declivity, which varies from 10 to 20 feet. The top generally slopes forward as at *x*, but sometimes it forms a cavity 6, 8, or 10 feet deep, as at *y*. The breadth of the terrace, measured parallel to the declivity, sometimes exceeds 100 feet. It will be seen that the form of the terraces has a very close resemblance to that of lateral moraines, as shewn in fig. 2, allowance being made for the effects of meteoric action during many thousand years. If this explanation of their origin is objected to, it will not be easy, I think, to account for their existence. Their marked deviation from the horizontal position will not permit us to suppose that they were deposited by water.

I was unable to trace them farther north than to a point behind Mr Napier's villa. Their absence from the part of the hill marked *a* seems to be well accounted for by its low elevation (600 feet), and the slightness of the declivity on its upper part. For moraines derive their materials from ledges or scarps of rock high above the glacier. If the rock is naked, fragments are detached from it by frost, rain, and wind; if covered with snow, the snow slides down occasionally in avalanches, bringing debris with it. The moraines first appear under the part *b*, which is 900 feet high, and steeper in the sides than *a*, and they are continued under *c*, which is more than 1200 feet high. That the higher ones, 1 and 2 may have extended as far south as the lower, or still farther, is probable enough. The surface under *c* is much broken, and something like vestiges of terraces may be found on it. The greater height of the hill here would render the torrents, which are very numerous, more destructive; and it is possible that the lower moraines were chiefly formed from the wrecks of the upper. The hills on the west side of the loch scarcely exceed the height of *a* in figure 1, and they present no distinct traces of lateral moraines.

Terminal Moraines.



The projecting tongue of land at Row, *i*, fig. 5, is about a mile in length at low water. It terminates in a point, marked by a pole or beacon, and its breadth at the other end where the road crosses it, near the hill foot (at *h o*, fig. 1) is 600 feet. Its elevation above the high tide level is 40 feet. There is a point at Roseneath, exactly opposite (*h* in fig. 5), whose breadth is greater, and its length smaller, but its height and materials the same. The space of open sea between the two points, at low tide, I estimated at 800 feet, and a shoal extends from the one to the other, upon which there is only 4 fathoms of water, while the depth a little below is 9 fathoms, and a little above, 18.

The small diagram P in the corner of figure 1, shews the form of Row Point on the cross section about midway between its extremities. The south side *g h* slopes very gently, while the north side *e f* is highly inclined. A drab-coloured clay is seen on the west side, and seems to form the basis of the

Point (*c* in the figure), above which are sand and gravel (*n*), not stratified, or only stratified near the top where the matter had probably been re-distributed by the tide. A few stones, from one to two feet in breadth, were visible near the east end. No rock is visible anywhere.

The clay, which is very fine, and slightly mixed with minute chips of slate, carried my thoughts to the "fine *flour* of rocks ground in the ponderous mill of the glacier betwixt rock and ice," which Professor Forbes tells us is poured out by the stream at the lower end of a glacier. The same drab or light brown clay covers the slopes of the hills to a considerable height, and most probably coats the whole bottom of the loch, for when the torrents are in flood, and sweep away portions of the stones on the beach, this unctuous clay is always found below them. Now, the friction of a glacier on the bottom and sides of the valley containing it, produces a clay of this description, much of which must remain in its bed after the ice has disappeared. The glacier also throws out portions of it at its lower end, along with the pebbles and stones by which it was enabled to scratch and groove the rocks beneath it. The materials, therefore, of the bank of gravel crossing the loch at Row, are such as we would expect to find in a terminal moraine. The form is also such as the terminal moraine of a glacier occupying all the upper part of the loch would naturally assume; that is, the side towards the glacier is steep, while the opposite one slopes away gently.

The northern or steep side does not present a uniform acclivity. There is an indentation at *f* which seems to me to correspond with the ancient beach found all round the shores of the loch, and was, I presume, produced by the same cause, namely, the action of the tide when the sea stood at a higher level by 30 feet.

But the remarkable uniformity in the height of Row Point *i* (Fig. 5), Roseneath Point *h*, and the broad point *k*, on which Roseneath Castle stands (40 feet above the high water line), induce me to think, that from whatever quarter the masses of sand and gravel seen on each were derived, the tide must have rearranged them, and levelled the surfaces to a greater or less extent. When this took place, the sea must have stood

45 or 50 feet higher (that is, the land stood so much lower) than it now does. Small portions of an ancient sea-beach or bottom, at an elevation of 40 or 45 feet, are visible also at Greenock and Gourock; and I have seen them at Bunaw and Connel Ferries on Loch Etive, as well as other parts of the west coast. The traces of these, however, are neither so well marked nor so numerous as those of the 30 feet beach, which I have met with at nearly every part of the east and west coast I have examined. If the action of the tides at an elevation of more than 40 feet above the present level, endured for a long period, it must have greatly changed the form of the gravel bank at Row. Supposing that the glacier retreated gradually, and formed a succession of narrow terminal moraines one behind another, they might merge into a single broad one under the continued action of the tides. There were probably other moraines at *x* (fig. 5), where the sea is only 2 or 3 fathoms deep at low water.

Boulders.

A great number of boulders are scattered over the surface, as indicated by dots on the figure. They are partly rounded, partly angular, and of all sizes up to 15 cubic yards, or 30 tons weight. They are chiefly of the mica-slate and chloritic rocks of the district, but mingled with them are some of granite, syenite, and greenstone. Of granite I saw about a hundred, of eighteen inches or more in breadth, including thirty, each of which measured a cubic yard or more. The highest of them were at an elevation of 600 feet above the loch. Of mica-slate boulders, there is one on the beach opposite Mr Napier's gate, 14 feet in height, measuring about 40 cubic yards, and weighing consequently 80 tons. Another, near the farm of Blairnig, is 15 feet long, 10 broad, 4 thick at the one end, and 2½ at the other, measuring about 15 cubic yards. It lies on an extensive tract of level ground about 400 feet above the loch. A great proportion of the boulders do not rest upon the rock, but upon the clay or gravel which covers it. Blocks of eight or ten cubic yards are seen sticking in the terraces, with soil below them.

The boulders are not found in lines, but scattered irregularly over the surface. They are most numerous at or near the beach, as may be seen on the west side of the loch, where, from the absence of buildings, few have been used up. On the highest part of the hill *c*, they ascend to about 800 feet of elevation. Above that height there are scarcely any visible; but some may have been broken up for fences. The summit of the ridge *b*, seems also nearly bare of boulders.

The granite boulders, which are all much rounded, could not come from any spot nearer than Ben Cruachen, 30 miles northward. Those of greenstone are generally deprived of their angles, and must have travelled some miles, since that rock, though existing in veins a few miles northward, nowhere appears *in situ* on the shores or sides of the loch. The mica-slate blocks, also, must have been brought to the places where they rest by some agent, for, except at a few points on the beach, there are no precipices from which they could roll down. Moreover, we find blocks of the proper mica-slate resting on the strata of quartzose chloritic rock with which it alternates; blocks of the latter resting on strata of the former; and at Row, blocks of both resting on the clay-slate. All these facts prove that the boulders have been moved, and that their motion was from north to south.

Now, let us suppose a glacier, having its source in the mountains beyond Loch Long and Loch Goyle (M and Y in fig. 5), to occupy the valley of Gareloch, its motion would be southward; it would transport the largest masses we have described in that direction; if it diminished gradually, it would leave a succession of terraces (moraines) like 1, 2, 3, 4, and scatter blocks over the surface; by friction on its bed, it would form a slimy clay, such as we find coating the beach and declivities; it would deposit a bank of moveable matter at its lower end, like the point at Row; finally, it would abrade, smooth, and round off the rocks, and leave scratches and groovings on their surface, running parallel to the axis of the valley, precisely such as we see at Gareloch. We have, then, not merely a few, but all the distinctive marks of glacier action at Gareloch, forming in the aggregate a body of evidence which seems to me to exclude the supposition of any other agency.

Glacial Phenomena on Loch Long and Loch Eck.

At my former visit I had seen marks of abrasion on the rocks at the upper part of Loch Long (M, Fig. 5), but no distinct striæ. On this occasion I found well-marked striæ in the lower part of the loch (L, Fig. 5), at a hamlet called Letter (*c*), nearly opposite Ardentinny, and within the high-water mark. They were upon a very compact rock, and ran south and north, or parallel with the sides of the valley. There was one groove four feet long, three-quarters of an inch wide, and rigidly straight. There were several others parallel to it, and deeper, but shorter, and among them one three inches wide. Abraded and smoothed rocks were seen abundantly elsewhere, but their texture was too coarse to exhibit striæ. A friend of mine saw similar striæ on the west side, at some distance south from *b*.

In a journey along Loch Eck (E in Fig. 5), I met with magnificent specimens of abraded, or, to use Sir James Hall's term, *dressed* rocks. The numerous little eminences at the foot of the lofty ridge of mica-slate *n o*, have their angles and projecting points ground off, and their surfaces smoothed and rounded, in a manner which must strike even persons unaccustomed to geological speculations. These hillocks present fine examples of the *domes arrondis* and *roches moutonnées*, or smoothed and rounded knolls, found in the glacier valleys of Switzerland, and which are admirably represented in the 6th, 8th, 9th, and 15th plates of Agassiz's work. One side of these hillocks, however, frequently remains in the rough state, namely the south, and this shews that the motion of the abrading agent (the glacier) was from the north. The polishing on the sides of the mountain is easily seen to the height of 300 feet, but it no doubt extends much higher. I examined the surface of one of the *roches moutonnées*, which was about fifty feet high, in the hope of finding striæ, but saw none. The texture of the rock, however, was coarse and partially decomposed. On more compact specimens I have no doubt that striæ may be discovered, and that they will point south and north.

In my article of 1st October 1845, I remarked that the hills on the east side of Gareloch had dressed surfaces to the height of more than 1000 feet. I found no distinct striæ, however, at a greater elevation than 310 feet; and while admitting that the action of a glacier might extend to this height, or somewhat beyond it, I was disposed to attribute the abrasion of the tops of the ridges and all the higher portions of the declivities to icebergs. On this occasion, however, I found striæ on the top of the ridge *d m* (Figs. 5 and 6), right west from the farm of Little Rahaen, and at 600 feet of elevation. There is an area of beautifully dressed rock here, about a quarter of a mile in breadth, nearly level, and thinly covered with peaty turf (*d* in fig. 5, and in the section below it). The covering has been washed off at many points, and the rock is seen as smooth as a hearthstone. At one spot I found a distinct groove half an inch broad, 3 feet long, and quite straight. The bearing was N. 30 W., which is very correctly that of the ridge, and of Gareloch valley. There were two others beside it, but fainter and shorter. The value of the groovings is, that they indicate *the precise direction* in which the abrading agent moved, and the indication in this case is important. Groovings in a deep and narrow valley like Gareloch, whether cut by a glacier, an iceberg, or a current—supposing a current capable of doing it—might be expected to have a general correspondence with the axis of the valley, because its steep sides would compel the agent to move in a determinate direction. But here the groovings occur on the very summit of the ridge, where the agent was free to move in any direction, and they indicate a motion in the very line which stony masses driven along by a current would most certainly have avoided—a motion *right along the crest of a ridge*, with a steep declivity on each side, towards which heavy bodies would inevitably have tended. It is conceivable, however, that a very large iceberg, moving steadily in a determinate direction, either in consequence of the immensity of its mass, or from being pressed on each side by fixed or floating ice, might groove the rock in this way. But I think there is a simpler and more satisfactory solution of the problem. Rocks striated horizontally are found in the Alpine valleys, 1500 feet above their bottom.—(*Forbes' Travels* *in the Alps*, p. 53, first ed.) May not this fact authorise us to conclude, that the same agent which dressed and grooved the bottom of Gareloch valley dressed and grooved the tops of the hills on both sides of it; and that glaciers, or moving masses of ice, not only filled the hollows, but enveloped all the ridges here to the height of 1500 or 2000 feet? There are five or six ridges between the south end of Loch Eck, and Luss on Loch Lomond, all nearly parallel, and running transverse to the laminar structure of the mica-slate. The ridge *o*, the southern part of *n o* (Fig. 5), the whole of *d m*, and of *g*, are examples. The straight or gently undulating outline of their crests, and the evenness of their sides, contrast, in a remarkable manner, with the rugged and serrated forms of the mountains to the northward, and seem to me clearly to indicate that they had been subjected to abrasion, by agents which either had not operated on the others, or had operated very feebly. One of these smoothed hills near Loch Lomond is at least 2000 feet in height.

Cause of the Cold of the Glacial Epoch.

Poisson, an eminent French mathematician, proposed an ingenious theory to account for the more intense cold which anciently prevailed in several parts of Europe, as evinced by the phenomena I have described, and many others. It has been deduced from observations made by the late eminent Prussian astronomer Bessel, that our sun, with the planetary system attached to him, is moving through the celestial spaces, in a determinate direction, at the rate of 3,800,000 miles per day.—(*Humboldt's Cosmos*, p. 152, Eng. ed.) Now, as the stars, which are the sources of heat, are very unequally distributed in the heavens, Poisson thought that the solar system, in its journey towards the constellation Hercules, might pass through spaces of very different temperatures, and that at some ancient and remote period it might have passed through a region of the heavens much colder than that in which it is now moving.

A much simpler explanation of the change has been proposed by Mr Lyell. Founding on principles developed by Humboldt, he observes, that the climate of any part of the globe depends, in a great degree, on the distribution of sea and land. The east side of all extensive continents in the extra-tropical regions, has a warmer summer and a colder winter than the western. The extremes of heat and cold, for instance, are incomparably greater in Lower Canada than in the Oregon territory, though they are both in the same latitude. Now, if North America at one time extended much farther eastward—if, for instance, it occupied all the portion of the Atlantic between Newfoundland and Britain—in that case, it is certain that Britain would have had the inhospitable climate of Labrador, or even one still more severe, like that of Greenland. There are various facts which point to the state of things here put hypothetically. Thus, the fresh-water strata of the Wealden group of rocks, from their extensive range and great thickness, imply that a river as large as the Mississippi had its estuary in England, and such a river could not exist unless a tract of land 1000 or 2000 miles in breadth, in connection with the British Isles, had occupied the eastern part of the Atlantic.—(See *Lyell's Elements*, i., p. 431.) Again, the same able geologist found evidence in the carboniferous rocks of North America, that the coarser materials composing them came from land lying to the eastward, and now covered by the Atlantic.—(*Travels in North America*, i., p. 86.) Finally, Professor Edward Forbes, in a most interesting memoir recently published, has shewn, from the relationship between the Fauna and Flora of the British Isles and of North America, that either the one has derived a certain portion of its animals and plants from the other, or that both have derived them from land now sunk in the intervening ocean.—(*Memoirs of the Geological Survey of Great Britain*, vol. i., p. 336-402.)

On the Use of the Microscope as an aid in Chemical Inquiry.

By John Davy, M.D., F.R.S., L. & E.; Inspector-General of Army Hospitals.*

That the microsope may be an important aid to the chemical inquirer, I believe will be readily admitted by all those who have made trial of it, if tolerably familiar with its use, and in the habit of employing a good instrument. This last condition is essential. To be eminently useful in research, an instrument of the best construction should be employed —an achromatic one, with a magnifying power of at least three hundred diameters.

Such an instrument may be considered as a new power to the chemist; making clear what was obscure; allowing him to distinguish by sight what, without such aid, he could determine only by complicated processes of analysis; enabling him to distinguish, in a mixture of animal and vegetable substances, various tissues and forms, and often in an instant satisfy himself of their nature; and equally so to satisfy himself by simple inspection, whether in the preparation of chemical substances, either by sublimation or precipitation, they are pure and unmixed, or impure and mixed with saline matter, or substances of a different kind thrown down or sublimed at the same time.

Taking this view of the application of the microscope, I cannot but think that the time is not far distant when the philosophical chemist will require it as much as, and even more frequently than, the balance; and that the one will be considered as essential to a laboratory of research as the other; and to the inquiring traveller, limited as to apparatus, more useful than any other single implement that can be mentioned hitherto attainable.

It may not be amiss to give a few examples for the purpose of illustration; making a selection from instances which have come under my own observation, some of which may be new.

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* Medical Gazette.

Preparatory to entering on a chemical examination, it is a desideratum to become generally acquainted with the nature of the subject of it. Such preliminary information the microscope is admirably fitted to afford.

It is a disputed point whether those peculiar, and, in many respects, singular birds,—the humming-birds,—feed on insects or the sweet juices of flowers; some naturalists maintaining that they live exclusively on the one, others that they live exclusively on the other. By repeated observation, first microscopical, afterwards chemical, made on the contents of their minute stomachs, I have ascertained that insects are the solid food, and that the sweet juice of the nectary of flowers is the ordinary drink of these birds. The tongue of the humming-bird, projectile and bifid, is peculiarly fitted for taking insects; and when moist with a honied viscid lure, its power is even increased. In every stomach of this bird that I have examined, I have detected with the microscope parts of insects, and sometimes entire and living ones; and I have found their remains also in the lower intestine.

Another instance may be given from the urinary excrement of insects. In the majority of cases in which I have examined this excrementitious matter, I have found it to be principally the lithate of ammonia, commonly in the form of granules, little exceeding in size the blood-corpuscles of man. In one instance, however—that of the excrement of a small night-moth, subjecting it first to microscopical examination—there were observed, mixed with the granules, well-formed crystals, resembling those of lithic acid, and which were proved to be of lithic acid by their chemical qualities, testing them chemically, and observing the results with the same power, for they were far too minute even to be seen with the naked eye.

Soils, sands, marls, afford other good examples of the use of the microscope as a preliminary to chemical examination.

Amongst the valuable deposits in the island of Barbadoes, distinguished for its extraordinary fertility, which it owes mainly to the nature of its soils, are beds of calcareous marl. Under the microscope, this marl appears to be almost homogeneous, except that amongst the molecules of which it is composed minute crystals are to be seen—short slender prisms, as of phosphate of lime; and, chemically examined, it is found to contain phosphate of lime, its chief ingredient being the carbonate, with a trace of silica.

The fertile soils of this very fertile island have been supposed to be deficient in this last mentioned substance. When subjected, however, to the microscope, the opinion is shewn to be erroneous. In most of them silica is found in abundance, either in the form of fine quartz sand, bearing marks of being water-worn, or in excessively minute angular fragments, probably of volcanic origin, or as the petrifying matter of infusoria. And chemical examination confirms this, proving, in addition, that silica is largely present in combination with alumina.

The sands of Barbadoes vary much in their nature, according to the situation in which they are found. The sea-sand, from two-thirds of the shore constituting the circumference of the island, consists of comminuted shells, water-worn, with very little siliceous or other matters, forming a valuable material for the purpose of agriculture, although hitherto little used. From the remaining one-third of the shore, viz., from the north-east coast, the sand is a mixture of quartz, in water-worn grains, and of comminuted shells, the former preponderating. The shell-sand corresponds to one kind of geological formation, the siliceous to another; the one peculiar to that part of the island, the greater portion of it consisting of calcareous deposits abounding in marine organic remains; whilst the siliceous sand corresponds to the smaller portion, formed of totally different deposits, partly chalk, containing silicated infusoria, partly, and in larger proportion, of clay, sand, and gravel, more or less cohering, with which beds of coal are intermixed, the former seemingly the detritus of primitive and transition rocks. Under the microscope, using a low power, the appearance of these two kinds of sand is distinctive. The shell-sand is merely diaphanous, whilst the quartz-sand is transparent, not to mention other peculiarities; and in mixture, the grains of each are most easily distinguished, so that even without chemical examination, on mere inspection, it may at once be pronounced which ingredient is in largest proportion. The same decision, from the like inspection, may be confidently made on some of the sands at a distance from the coast, which consist almost entirely of siliceous matter, without any traces of comminuted shells; and, in each instance, chemical examination is found to be confirmatory of the microscopical.

I shall pass now to some instances of a different kind. The unaided vision, as it is well known, cannot distinguish, with sufficient exactness, precipitates of different substances by their appearance, when they are colourless, as those of the different earths, whether thrown down in the state of hydrate, or in combination with an acid. But, calling in the aid of the microscope, the distinction can often be made without difficulty; and, if not at once, it can be made with little trouble by the addition of an acid, of one, namely, the combinations of which with different earths, under the microscope, have appearances that are characteristic of them.

Alumina and phosphate of lime,—the one an almost universal ingredient of soils, the other a rare ingredient, excepting in an exceedingly minute quantity,—are, as is well known, both precipitated from solution in acids by ammonia. To determine which constitutes the precipitate, so important in relation to agricultural purposes, is a difficult matter, employing merely chemical means as tests, unless in a well appointed laboratory. But with the aid of the microscope the difficulty vanishes. Seen with it, the precipitate of alumina, owing to its excessive subtlety, and the low refractive power of its particles, has a hyaloid appearance, while the other has a very fine granular aspect. The addition of a drop of dilute sulphuric acid will make the appearances even more distinct. If the precipitate is entirely aluminous, it will have a glacial appearance, and, on evaporation to dryness, this appearance will be retained, the crystallizations formed being not unlike those of ice in their general character; whilst, if the precipitate is of phosphate of lime, immediately on admixture with the acid, if properly proportioned, needle crystals will appear in increased quantity mixed with some delicate rhomboidal plates. And, in the instances of mixed *precipitates* by ammonia, of alumina and phosphate of lime, according as the one or the other predominates, will be the proportion in which the one or other aspect will be imparted to the mixture; and, should phosphate of magnesia be present in admixture with alumina and phosphate of lime, it may easily be detected under the microscope. Before the addition of the acid, its well-marked crystals will be seen, which after that addition will disappear.

As my object is chiefly to draw attention to the application of the microscope as an aid to chemical research, the instances adduced, I trust, will be considered sufficient in the way of illustration. Very many more might be given, but they would be out of place on this occasion. Were they minutely detailed they would be tedious; and were they only briefly adverted to, practically they would be useless. The subject, to have justice done to it, would require a distinct treatise, founded on laborious research, and giving the results of such research; and it is one, of which it may confidently be predicated, that it would amply repay whatever time, whatever labour, might be expended on the undertaking.

In conclusion, this, it appears to me, may justly be said in commendation of the microscope, in all its applications, that its tendency is equally to facilitate and increase accuracy of observation, and hence to remove vagueness and give precision of views, and, at the same time, to add wonderfully to the interest of inquiry, and to extend its sphere,—in this respect having an influence in relation to subjects for chemical research resembling that of the most powerful telescopes in relation to the objects of astronomical research, both conducing, the one hardly less than the other, by the phenomena they display, to excite in the mind of the philosophical inquirer feelings of admiration, and of awe, and not less of humility.

BARBADOES, *January* 19, 1846.

On the Quantity of Fallen Rain in the Lake Districts of Cumberland and Westmoreland. By J. F. MILLER, Esq. Communicated by the Author.

Notes.

At the close of October, Gatesgarth and Wastdale exceeded Grasmere by 17·16, and 5·40 inches respectively; but the excess in the latter locality in November and December has turned the scale as regards Wastdale, and reduced the excess at Gatesgarth to 3 inches. The fall at Langdale, during the seven months the instrument has been up, is only 10½ inches short of Seathwaite; and, taking the other five months in the same proportion, gives 136 inches for the whole year.

At Seathwaite, there have been thirty-one days in which the fall was between 1 and 2 inches, five days between 3 and 4 inches, one day between 4 and 5 inches, and one day between 6 and 7 inches.

On the 27th November 1845, there was measured at Seathwaite 6·62 inches, and on the 26th and 27th nearly 10 inches, being the greatest quantity of rain which has ever been measured, in the same period, in Great Britain. At Langdale Head, in Westmoreland, the fall on the 27th was 6·28 inches, and on the 26th and 27th nearly 9 inches. The consequence was, the heaviest flood which has occurred at these places for at least sixty years past. Windermere Lake had not been so high for the last fifteen years; on the night between the 26th and 27th it rose 2 feet in perpendicular height; the quays along the banks of the lake were immersed in water, and much wood was carried away by the current and lost. Keswick Lake had not been so high since November 30, 1838. Of the total quantity of rain measured in the vale of Borrowdale in 1845, 106·58 inches fell in the six months of January, March, August, October, November, and December, and nearly 46 inches in the two latter months.

Such was the violence of the storm on the night of the 28th December in the lake districts, that a number of fish were found next day on the margin of Bassenthwaite Lake, which had been thrown up by the waves in the course of the night by the force of the wind, a circumstance wholly without a parallel, except on the night of the memorable 6th of January 1839. The rain which fell in the preceding twenty-four hours amounted to 4·22 inches; at Whitehaven, the quantity was 323, or about a quarter of an inch.

Through the kindness of various gentlemen, I am enabled to add returns of the rain-fall in 1845, from several places throughout Great Britain, by way of comparison with the quantities measured in the lake districts.

		Inches.		Inches.
Allenheads, Northumberland,		56·411	Leeds, ...	25·586
Kendal,	Westmoreland,	53·346	North Shields, . .	26·200
Cartmel,		53·665	Culloden, Inverness, N. B.,	27·632
Rampside,		40·289	Applegarth Manse, near Lockerby, ...	30·320
Tirril,		40·000	Arbroath, Cupar, Coupar-Angus, N. B., . .	28·211
Bolton-le-Moor, .		48·110	Liverpool, . . .	34·06
Carlisle, ...		31·280	Stratton, Cornwall, .	40·89
Westmoreland, Brougham Hall, ...		35·000	Uckfield, Sussex, . .	25·03
Manchester, . . .		41·415	Empingham, Rutlandshire,	24·61
Doncaster, ...		29·198	Helston, Cornwall, . .	37·80
Highfield House, Nottinghamshire, . . .		29·595	Kelso, Roxburghshire, N. B.,	24·42
Cirencester, . . .		28·970	Makerstoun, near Kelso,	21·27

The fall at Seathwaite is more than three times the quantity measured at Whitehaven, one of the wettest *towns* in the kingdom. It exceeds the fall at Leeds by six times; at Culloden by five and a-half times; at Doncaster and Highfield House, Nottinghamshire, by five times; at Cirencester and Arbroath by five-and-a-quarter times; and at Makerstoun, near Kelso, the seat of Sir Thomas Brisbane, Bart., by more than seven times. Seathwaite exceeds Doncaster in January by fifteen times, in November by twenty-one times, and in December by nine-and-a-half times. It exceeds the quantity at York in January by 16 inches, or twenty times; in March by nine times, and in November by twenty times. It exceeds Dublin, in March by fourteen times, in April by thirteen times, in October by five times, and in November by seven times.

The quantity measured at Seathwaite, and at Langdale Head in the month of December (24·02 inches), is moe than falls at some places in England during a year.

Remarks.

It is much to be regretted, that the meteorology of our lake and mountain districts should have been so long neglected. Prior to the establishment of these gauges, there were none stationed in Cumberland, and, so far as I am aware, only two among the lakes of Westmoreland, viz., one at Esthwaite Lodge, and the other at Grasmere. The largest quantities measured in any year were, 86 inches and 90 inches respectively; and so startling did these amounts appear to meteorologists when first made known, that many were led either to doubt their authenticity, or to suspect the accuracy of the instruments employed. But subsequent investigation shews, that these values are exhibited in some portions of the lake district of Cumberland only in the very *driest* years. Thus, in the period from July 1844 to June 1845 (which for drought has only found a parallel in the memorable 1826), the fall at Gatesgarth and Wastdale Head amounted to 83·96 and 88·42 inches respectively; and at Grasmere, in Westmoreland, to 74 inches nearly. But we suspect that meteorologists will hardly be prepared for the discovery, that we have localities in our own country which, even in average years, exceed the amount of rain annually deposited in many tropical climates. Yet such is the almost incredible fact. At Grenada, lat. 12°·5′, the average fall is 126 inches; at St Domingo, lat. 18°.20′, it is 120 inches; and at Calcutta, in lat. 22°·35′, it is 81 inches.

In the past year, the quantity measured in the vale of Borrowdale exceeds the largest of these amounts by 25·87 inches.

An inspection of a map of the county in connection with the table will shew, that the wettest portions of the lake district are those situate at the head or eastern extremities of those valleys formed by our highest mountain ridges, amongst which are the Great Gabel, Sca Fell, Glaramara, Red Pike, High Style, and Honister: the first being apparently the grand central point of attraction and condensation for the warm vapour arriving in a south-westerly current, across the Atlantic; and it is a remarkable coincidence, that nearly all our lakes bear in the direction of Gabel, so that, if extended onward in a direct line, they would all converge at the base of this noble mountain.

Immense as is the deposit of rain at Gatesgarth, Grasmere, Wastdale, and other portions of the district, as shewn in a former table, even these enormous quantities sink into comparative insignificance when compared with the fall at Seathwaite, a small hamlet at the head of the vale of Borrowdale. It exceeds the wettest of the other localities by 27·74 inches, or by one-fourth nearly. Now it is chiefly the deposit in the vale of Borrowdale which supplies the majestic river Derwent, and the extensive and picturesque lakes of Derwent and Bassenthwaite, so that we might, *a priori*, have expected to find the greatest amount of rain in this section of the district.

The great difference in the fall between places closely contiguous to each other is very remarkable. The proportion which obtains between Ennerdale Lake and a farm-house about 1½ mile distant, is as two to one nearly. Loweswater, Buttermere, and Gatesgarth, are all in the same line of valley, surrounded by the same ridges of mountains, and are each distant about two miles from the other. Buttermere exceeds Loweswater by eighteen inches, or one-fourth; and Gatesgarth, at the head of the valley, exceeds Buttermere by 36·65 inches, or nearly one-half. Here the difference between the head and foot of the valley, in a distance of four or five miles, is 54·588 inches. But the sudden increase in the fall towards the head of the valleys, is appreciable at much more limited distances. At Wastdale Head, I have two gauges of precisely the same size and shape, and within half-a-quarter of a mile of each other, yet the difference of the receipts in a single month sometimes amounts to half an inch.

The annexed statement will shew, that the excess is always in favour of the higher gauge, marked No. 1.

1845.	No. 1.	No. 2.	Diff.
October, .	12·35	11·89	·46
November, .	12·31	11·90	·41
December, .	16·18	15·78	·40
1846.			
January, .	12·97	12·47	·50
February, .	6·60	6·58	·02
March, .	10·35	10·07	·28
April, .	6·59	6·16	·43
May, .	3·65	3·44	·21
June, .	5·33	4·85	·48

The current of vapour is apparently only partially decomposed in passing over a flat, or even an undulating country; it aims at once at the loftiest heights, passing over the less hilly districts with little diminution of its original weight or volume. But, on reaching the mountain peaks, the sudden change of temperature causes a rapid and continuous condensation in the form of vast torrents of rain, whilst comparatively little descends on the adjacent plains.

As an instance of the low temperature on our mountain tops, I may mention, that on making the ascent of Skiddaw, on the 5th of September last year, the thermometer on the summit stood at 41°; sky overcast, the sun shining out at intervals; the temperature of the air at the foot of the mountain at 3^h 30^m, was 58°. Snow not unfrequently continues on Sea Fell till the middle or end of June. We remember seeing a patch on the 15th of June 1843; and on the neighbouring mountains the air was so intensely cold, that we think it could not be more than two or three degrees above the point of congelation.

That the rapid increment in the fall in approaching mountainous districts is owing to the causes above alluded to, and not to the greater number of wet days, is evident on an inspection of the table, where it will be found that we have as many wet days at Whitehaven, near the level of the sea. Indeed, it rarely rains in any part of the lake district, that the day is not also wet, more or less, at the coast; and, in comparing the number of wet days at various places, we not unfrequently find them to obtain in the inverse ratio to the fall of rain. Thus, in 1845, they range from 195 to 211 in the lake districts; but at Manchester, with a fall of 41 inches, they amount to 235; at Culloden, with a fall of 27 inches, to 237; and at Kendal, where the quantity of rain is 53 inches, the wet days are only 178. At Carlisle, the wet days are the same as in the lake districts, where the fall is four times as much.

We are informed by a gentleman recently returned from India, who was many years medical attendant to the Rajah of Sattarah, that he seldom measured more than 40 inches of rain in the plain; but among the hills, 30 miles distant, the annual quantity reached 350 inches, and as much as 9 inches had been known to fall in 24 hours.

The utility and beauty of this arrangement is obvious, since the mountain torrents afford a continuous supply of water to the lakes and rivers, which otherwise could scarcely have an existence. The rivers thus called into being aid the efforts of the husbandman, by carrying off the superfluous moisture from the plains, which, without such a provision, would be in danger of stagnating into pestilence.*

J. F. MILLER.

WHITEHAVEN, *July* 1846.

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* The month of July 1846 was very wet; at Whitehaven, the fall was upwards of nine inches, being the greatest we have had since January 1834. In some parts of the Lake districts, the quantity reached 20 + inches.

The five mountain gauges, which are at elevations varying from 1200 to 3166 feet, would seem to shew that the rain *increases* from the valley upwards, to an altitude of 2000 feet, and decreases above that elevation. We have continuous results since March last, and they promise to be both highly interesting and important to meteorological science.

Fall of Rain in the Lake District of Cumberland and Westmoreland in the year 1845.

1845.	White-haven.	St James' Steeple, 78 feet above Street.*	The Flosh 2½ miles south of White-haven.	Cocker-mouth.	Kes-wick.†	Enner-dale Lake.	Lowes-water Lake.	Butter-mere Lake.	Gates-garth.	Wastdale Head.	Grasmere.	Langdale Head.	Seath-waite, Borrow-dale.
Jan.	4·541	2·767	4·88	3·195	6·426	7·560	7·100	9·53	13·78	11·00	15·023		16·81
Feb.	2·830	2·025	2·76	1·045	1·004	2·160	1·897	1·70	2·61	3·40	2·252		3·48
March	3·735	2·850	4·24	3·895	3·978	6·715	5·814	7·06	9·07	10·37	10·256		13·21
April	2·587	1·770	2·67	2·330	4·886	5·500	4·859	7·31	10·60	7·40	6·133		10·57
May	1·480	·995	1·74	1·970	2·270	3·200	2·629	3·62	4·90	4·13	2·289		4·57
June	4·099	3·172	3·86	3·090	4·032	5·550	4·954	5·97	8·02	6·15	5·010	7·14	8·25
July	2·900	1·980	3·52	2·600	4·112	5·050	5·274	6·39	8·72	5·42	6·191	7·65	8·65
Aug.	6·995	5·100	7·22	8·720	8·806	10·005	8·858	11·17	15·75	12·31	10·185	13·01	15·61
Sept.	3·653	2·405	4·11	3·825	5·004	5·480	4·576	6·54	8·49	7·53	6·538	8·55	9.77
Oct.	6·744	4·853	7·35	6·595	5·948	8·745	7·446	8·20	10·88	12·35	11·785	13·41	15·17
Nov.	4·022	2·350	4·40	3·095	7·140	6·740	6·966	8·50	13·48	12·31	21·732	18·84	20·84
Dec.	5·62	3·222	6·25	6·570	8·596	10·175	9·169	11·49	17·83	16·18	23·614	24·02	24·94
Totals	49·207	33·489	53·00	46·930	62·202	76·880	69·542	87·48	124·13	108·55	121·008	‡92·62	151·87
1844	36·723	27·862	39·31	...	40·629	54·626	...	...	...	...	65·417	85·05	Grasmere
												7·57	Difference.

* St James' Church is 49 yards distant, in a direct line, from the gauge in my garden.

† For the account from Keswick, I am indebted to Mr J. Otley, and for that from Grasmere, to the Rev. Edward Jefferies.

‡ Or 136 inches for the year.

Wet days in 1845.

1845.	Whitehaven.	Flosh.	Cockermouth.	Keswick.	Loweswater.	Buttermere.	Gatesgarth.	Wastdale Head.	Grasmere.	Langdale.	Seathwaite.
Jan.	21	16	19	21	21	21	21	20	22		22
Feb.	10	5	6	9	8	8	11	9	9		11
March	14	11	14	14	14	12	15	13	15		15
April	13	10	11	12	12	11	12	12	12		11
May	14	14	16	15	15	17	16	14	12		15
June	16	15	18	17	15	16	17	16	16	18	18
July	13	15	17	11	15	15	16	16	16	15	15
Aug.	17	20	21	20	18	21	19	21	15	20	22
Sept	15	12	17	13	13	16	16	17	14	17	15
Oct.	20	20	26	21	19	23	23	26	21	22	21
Nov.	17	15	21	19	20	18	19	21	19	19	20
Dec.	23	22	26	23	25	24	25	26	25	26	26]
Total days	193	175	212	195	195	202	210	211	196	137 From June	211 137 From June.

Note on "Sheep-manure." By JOHN DAVY, M.D., F.R.S., &c., Inspector-General of Army Hospitals.

Of the many indications of enterprise in the present times, not the least remarkable are the exertions which have been made to procure manure; and these have been displayed in the West Indies fully as much as at home, as indeed might be expected, considering the large capital invested in the culture of the staple article of the agriculture of these regions—the sugar-cane—and the importance justly attached to the supplying it well with manure. Amongst the various articles imported for this purpose, one has come to my knowledge, which I believe may be deserving of the attention of our own agriculturists, on account of its excellent qualities, respecting which there cannot be a doubt. I allude to sheep's dung in a dry state, dried merely by exposure to the sun and wind, and, therefore, retaining all, or almost all, its original virtues.

This manure has been imported into Nevis in casks, by an enterprising and intelligent merchant, a planter of that island, who is now applying it to his cane-fields. From him I have received a portion of it, sufficient for examination. I shall restrict myself at present to a very brief mention of it, and of the results obtained.

It is of a dark greenish-brown colour, a congeries of minute fibres and scales or laminæ. To water it yields slowly a solution, containing, besides a greenish-brown colouring matter, a little sesqui-carbonate of potash, carbonate of ammonia, and a trace of muriate of ammonia; acted on by dilute muriatic acid, a solution is formed containing a large proportion of phosphate of lime, and some lime and magnesia, which probably exist in the manure in the state of carbonates. The part resisting the action of water and acids, consists principally of vegetable fibre and silicious matter, and is not inconsiderable.

This composition, if I recollect rightly, agrees very nearly with that of the dung of our English Down-fed sheep, and is certainly such as is in accordance with high fertilizing powers. The article, I am informed, was imported from the Spanish Main, but from what part exactly of the American continent I have not learned, nor its price, nor in what quantities it can be procured.

Could it be had in large quantities, and at a moderate price, from a matter of curiosity—for it is hardly more at present in relation to British Agriculture—it might become one of some importance, especially as in its dry pulverulent state, it is so well fitted for drill husbandry, "to be drilled in with the seed," a plan which a writer in a late number of an agricultural journal* considers peculiarly advantageous, and, as it appears to me, very justly so.

BARBADOES, *October* 2, 1846.

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* Mr Henry Liddell. See "The Farmer's Herald" for August 1846, p. 90.

On the Course of Electric Currents on the Earth, and on the Cause of Terrestrial Magnetism. By GEORGE A. ROWELL, of Oxford.* Communicated by the Author.†

At the late meeting of the British Association at Southampton, Professor Oersted, speaking on the deviation of falling bodies from the perpendicular, stated, that experiments on the subject shew that there is a deviation towards the east, which agrees with theoretical calculation; but that there is also a deviation towards the south, which is not easily accounted for. In the discussion which followed, it was suggested that this phenomenon was caused by currents of electricity, which are said to circulate round the earth in the direction of the parallels of latitude, to which currents are attributed the phenomena of terrestrial magnetism; but it was considered doubtful whether such currents could cause a falling body to deviate towards the south.

The phenomenon in question will be more readily accounted for, if it can be shewn that there are currents of electricity in this hemisphere from north to south, the probability of which I endeavoured to prove in a paper read before the Ashmolean Society in 1840, and at the meeting of the British Association at Glasgow.

As the two theories are, at present, hypothetical, and as both cannot be correct, I will endeavour to shew, that facts preponderate in favour of the latter opinion, and that phenomena of terrestrial magnetism can be more readily accounted for by the latter theory.*

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* Read before the Ashmolean Society, Nov. 23, 1846.

† I beg respectfully to recall attention to the paper submitted to the Ashmolean Society, February 23 (which appears in the Edinburgh New Philosophical Journal for April last), in which I endeavoured to shew the probability that storms and aërial currents are owing to partial vacuums caused by the precipitation of rain, and the escape of electricity from clouds in a much greater degree than to changes of temperature.

This theory seems, in some measure, confirmed by the extraordinary rains on the European continent during the last month, being accompanied, in England, by violent storms of wind, chiefly from the west and north-west.

G. A. ROWELL.

The hypothesis that currents of electricity circulate round the earth in parallels of latitude, is grounded (chiefly, I believe) on the phenomena of thermo-electricity, from which it is assumed, that as the earth is unequally heated by the sun, there must be currents of electricity on its surface, the earth being thus a grand thermo-electrical machine; and as the magnetic needle takes a position at right angles to the conducting wires of a voltaic battery, that, therefore, these currents on the earth's surface must be at *right angles to the magnetic meridian.* This theory has been adopted by many of our most eminent philosophers; but I trust I may not be charged with presumption in pointing out some objections which appear to me to tell against it.

There has never, I believe, been any distinct proof, *by experiment,* of any currents on the earth crossing the magnetic meridian. The experiments of Mr Fox shew, that the currents he detected were generally from north to south, but it is probable the whole of them were owing to local causes.

Again, the intensity of terrestrial magnetism is weakest at or near the equator; whereas, if it be caused by thermo-electricity, it would probably be most intense where the sun acts with the greatest power; and it is also reasonable to suppose that there would be a great difference in the intensity of terrestrial magnetism during the summer and winter months in our own and higher latitudes.

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* Theories have been proposed from time to time, to account for the phenomena of terrestrial magnetism on the hypothesis that they are caused, wholly or in part, by some magnetic agency situated in the interior of the earth, or that the earth itself acts as a magnet.

It would be an interesting experiment to ascertain if there is any difference in the weight of unmagnetized iron in high and low latitudes, more than can be accounted for by the form of the earth; for if any of the phenomena of magnetism are caused by magnetic influence within the earth, it is probable there would be, if such opinion be correct, an increase in the weight of iron corresponding with the increase of the dip of the needle, or rather an appearance of increase of weight from the addition of magnetic attraction to the specific gravity of the iron.

The declination of the needle seems totally inexplicable on this hypothesis, and the influence of the aurora on terrestrial magnetism, especially in high latitudes, also seems to tell against the theory.

As regards the theory that there are electrical currents from the frigid regions to the equator, it is proved by Volta's and other experiments, that whenever evaporation takes place, the vapour carries off positive electricity, leaving the water negatively charged. The experiment of Professor Faraday, on the electricity of steam, from which he was led to conclude, that no electrical development could take place from the evaporation of a saline mixture, and that evaporation is not the cause of the electricity of the atmosphere, tell against the theory; but the conclusions of Pouillet on the electrical phenomena of evaporation are directly the reverse; for, although he agrees generally with Dr Faraday on the phenomena of the electricity of steam, he "has demonstrated that the conversion of pure water into vapour, at any temperature, is not attended with any disturbance of the electric equilibrium, but that vapour, rising from solutions, however weak, gives signs of electricity, varying in kind, according to the nature of the substance dissolved. From saline or acid solutions, the vapour carries up a charge of positive electricity, and leaves the solution in a state of negative electricity; and the rule was verified particularly with regard to solutions of sea-salt." The experiments of Armstrong and Pattison, on the electricity of steam, were with boilers without any arrangement for causing friction, and without regard being had to the purity of the water.

The experiment of Mr Pattison shews the vast quantity of electricity caried off by vapour, as he says,—"I repeated Volta's experiment, by placing a hot cinder upon the cap of a gold-leaf electrometer, and projecting a few drops of water upon it, when the leaves diverged strongly with negative electricity. I observed that when the cinder was very hot, and the production of the steam consequently very rapid, the electricity given out was always most powerful.

"I then insulated an iron pan, 12 inches diameter and

2 inches deep, and attached to it a pith-ball electrometer, with balls $\frac{3}{8}$ths of an inch diameter, and threads 5 inches long, and also attached to the pan a metallic wire, the pointed extremity of which was placed about $\frac{1}{20}$th of an inch distant from the point of another wire connected with the ground. The iron pan was then filled with cinders, very hot, from a wind-furnace, and on projecting upon them a few ounces of water, steam was evolved with great rapidity, and at the same moment the pith-balls diverged to the distance of an inch, and sparks passed between the metallic wires. This was several times repeated."—(*London and Edinburgh Philosophical Magazine*, 1840, p. 460.) And the experiment on evaporation from insulated and uninsulated vessels (an account of which I submitted to the Ashmolean Society in 1841)* tends to shew that electricity is a necessary agent in evaporation at moderate or low temperatures.

It is of little consequence, as regards the phenomenon in question, whether the vapour carries off the electricity, or whether (as I have endeavoured, in former papers, to shew) the electricity carries off the vapour; it is sufficient to know that, during evaporation, positive electricity is carried off, and the water left in a negative state; consequently, from the enormous evaporation which is continually going on in the equatorial regions, the earth there must always be in a negative state unless the whole of the vapour falls again in the same regions; but I believe it undoubted, that a great portion of the rains which fall in the frigid regions is from vapour carried by the *superior* trade-winds towards the poles; and if any more vapour falls in the frigid regions than rises from those parts of the earth, the earth there must be surcharged with electricity, which surcharge would naturally rush along the earth's surface towards the equatorial or negative parts of the earth. The analytical researches of Professor Forchhammer, on the quantity of saline matter in sea-water from tropical and frigid regions, appear fully to establish the fact, that "while the quantity of water carried away from the tropical sea is greater than that which rain and rivers give back to that sea, the reverse takes place in the *polar seas*, where the evaporation is very small, and the condensation of vapour very great." Therefore, if my views be correct, the earth is similar to a vast hydro-electrical machine. The vapour, with its electricity, rising at the equator, is carried *over* towards the frigid regions where the vapour falls, and its electricity escaping to the earth, rushes along its surface towards the equator, and is again taken up by the rising vapour. This circulation of electricity would go on uninterruptedly during the summer, or at all times when the air in the frigid regions is sufficiently damp to conduct the electricity from the vapour or clouds to the earth; but when the air in these regions is very dry during severe frosts, these currents are interrupted by the escape of the electricity from the vapour being prevented, when the electricity accumulating in the region of the clouds, would exhibit the phenomena of the aurora, either by flashing back through the upper and rarer air towards the equatorial or more temperate regions, or, by the accumulation of more vapour, charged, either more or less, with electricity, causing an electrical disturbance, through the whole mass of vapour by the rush of electricity to gain its equilibrium.

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* See note to a paper on this subject in the Edinburgh New Philosophical Journal, vol. xxxviii., p. 50.

That the aurora is connected with the formation of clouds, the observations of Parry, Franklin, Hood, Richardson, and others, have satisfactorily proved. The fall of minute particles of snow during an aurora in the zenith, tends to prove this, and also the theory of the electrical suspension of vapour. Dr Richardson says it is by no means an uncommon occurrence after an aurora has continued for some hours, and is *about to disappear*, but the particles are so fine that they are more readily felt on the face than seen.

I believe that the circulation of these *hydro-electrical* currents is connected with the phenomena of terrestrial magnetism; and the fact that all ferruginous matter lying in the line of the magnetic meridian becomes in time magnetised, tends to prove it, especially when it is borne in mind that a needle is rendered magnetic by a shock of electricity passing *through it*, and that the end of the needle, at which the positive electricity enters, becomes its north-pole (unless the needle at the time of the shock is lying in or about the line of the magnetic meridian, in which case the end pointing north becomes its north-pole). It is, therefore, probable, that these currents give direction to the magnetic-needle, and the interruption of these currents will account for the occasional influence of the aurora on the needle.

As the pressure of the air from the colder regions, owing to its superior density, is the cause of the trade-winds, the upper current, or *superior trade-winds*, must be from the equatorial towards the polar regions, and must be strongest to the *coldest* parts of the earth; thus rendering those parts the magnetic poles,* by carrying to them a greater quantity of vapour, and consequently electricity, than to other parts.

If more electricity be received by the earth at the magnetic poles than at any other part of the earth, the electricity in passing off towards the equatorial or negative parts of the earth would diverge east and west from the direct line of longitude, owing to the excess of electricity at the magnetic poles over other parts of the earth in *the same latitude;* the declination of the needle may be thus fully accounted for.

A large portion of the ice formed during the winter in some parts of the Polar regions, such as the neighbourhood of Greenland or Spitzbergen, must be dispersed during the summer by being drifted into lower latitudes; but the ice formed in winter in the higher latitudes of America is so blocked in by the land, that the greater part of it must remain unmelted during the summer season, thus rendering that part constantly one of the coldest in the northern hemisphere, and consequently one of the magnetic poles of the earth. The high latitudes above the European and Asiatic continents, being under similar circumstances, will account for the magnetic pole in those regions also.

The stronger influence of the American magnetic pole is probably owing to the vast extent of tropical seas in that meridian rendering the air rising there and flowing from thence to that pole highly charged with vapour, and consequently with electricity; while the great extent of land, in the meridian of the Siberian pole, may cause the air flowing over towards that pole to be comparatively dry, and consequently with little electricity.

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* Sir D. Brewster, some years ago, pointed out the connection between the magnetic poles of the earth, and the greatest degree of cold on its surface.

The change, observed in England, of the declination of the needle from 11° 15′ east in 1580 to its present westward position, has been attributed to a gradual motion of the magnetic pole from east to west. *I believe that no such motion has taken place, and that the magnetic poles have never changed their position in any considerable degree;* the change in declination has probably been caused by an increase of power in the American pole, or decrease of power in the Siberian pole, or perhaps a combination of both causes, thus gradually bringing England under the influence of the American or stronger pole.

It is obvious, that as there are two magnetic poles in the northern hemisphere, one causing a westward and the other an eastward declination, there must be a line of no-variation where the influences of both poles are equal (that is, a *diverging line of no-variation.*) This line of no-variation is now a little to the east of St Petersburg, and it must have been *this* line of no-variation which passed over London from *west* to *east* in 1657; for had the line of no-variation been caused by a motion of the poles from *east* to *west,* it would have been at Paris (2° 20′ east longitude) before it could be at London; whereas it was at Paris in 1666, or 9 years later than at London; and as a proof that it was a *diverging* line of no-variation, the variation at Bristol (2° 50′ west longitude) was 1° 27′ west at the time there was no declination at Paris, whereas it must have been an eastward variation, had the magnetic pole been at that time in the meridian of Paris.

It is difficult to assign a cause for the change of strength in the poles. It is not improbable that the increase of the American pole may be from an accumulation of ice in that part of the earth, or that the Siberian pole may have decreased in strength from an increase of temperature in those regions; or, from a decrease in the evaporation from the heated land in that meridian rendering the air flowing over to that pole more dry, and consequently reducing its electricity.

The daily variation of the needle is probably owing to the difference in the quantity of vapour rising from the earth during the night and day.

I believe that experiments with electrical kites, in the frigid regions, would throw some light upon this subject, and in submitting a paper to the British Association, 1840, I expressed a wish that means might be adopted to test the correctness of the theory, by electrical balloons; "for if electrical conductors could be raised to the *clouds in the frigid regions* during the severe frosts of winter, I believe it possible to cause the aurora, and perhaps with startling and extraordinary effects; and I have no doubt it would lead to very important discoveries in the science of magnetism: to shew the probability of which, I beg to direct attention to the observations of Captain Franklin, who found the aurora had great effect on the needle at Fort-Franklin; and those of Captain Parry, who found the aurora had no effect whatever at Fort-Bowen.

"This discrepancy is confirmatory of the theory: for as the magnetic pole is computed to be in 70° 0′ north latitude, streams of electricity rushing thence to the equator could not act on the needle at Port-Bowen, which is in 73° 4′ north, but would act on the needle at Fort Franklin, in 65° 12′ north."

Notices of New Localities of Rare Minerals, and Reasons for Uniting several supposed distinct Species. By FRANCIS ALGER, Member of the Boston Society of Natural History, of the American Academy of Arts and Sciences, &c.*

Phacolite from New York.—This rare mineral, which comes to us principally from Bohemia and Ireland, I have discovered among a suite of specimens of various kinds found on New York Island, near Harlem, by Messrs Mathews and Johnson, of New York city. The

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[*] Read before the Boston Society of Natural History, January 7, 1846.

specimens, which eventually proved to be this mineral, were labelled Stilbite; but their appearance was so peculiar, that I questioned at the time whether they had been correctly designated, and determined to examine them carefully at my earliest convenience. I have since received two other specimens, better characterized than the first, from Mr Johnson. The crystals are in a geode form, implanted on calcareous spar, and associated with silver-coloured mica and a few scales of Oligisto-magnetic iron-ore. They are of a wax or honey-yellow colour, have a waxy lustre, and the smallest individuals are translucent. They are brittle, breaking with an uneven fracture, have none of the foliated structure of Stilbite, and afford no indications of cleavage; hardness superior to that of Stilbite, and equal to that of Chabasite. Their surfaces are roughened or pitted, so as to reflect no image by which they could be subjected to measurement by the goniometer. Before the blowpipe, a fragment of the mineral swells and intumesces slightly, like the Bohemian and Ferroe Chabasite, and fuses into an opaline, blebby bead; at the moment of ignition, in the outer flame, it gives out a beautiful green phosphorescence, which I have also noticed, in a less degree, in the phacolite from Ireland. It is soluble in hydrochloric acid. The crystals, at first sight, appear to be rounded, and to have no determinate form; but, on closer examination, some of the smaller and more isolated ones are found to be nearly perfect double six-sided pyramids, precisely similar to the phacolite from Bohemia, differing from it only in colour and lustre. I cannot doubt that, like that mineral, they are secondaries to a primary rhombohedron, probably of the same measurements, and are also identical with it in composition. The absence of well-defined cleavage is unfortunate, but this is a defect which applies equally to the foreign mineral. Nor is the rhombohedral cleavage of ordinary chabasite, of which phacolite is by many supposed to be only a variety, by any means easily determined; in fact, Sir David Brewster has suggested, from optical investigations, whether the primary form of chabasite be not a prism.

Is Phacolite a variety of Chabasite, or distinct from it?—Tamnau, of Berlin, in his very complete little essay on chabasites, has given very good reasons for uniting the two; while Breithaupt has maintained them to be distinct. The primary rhombohedron of phacolite, according to Breithaupt, is P on P, 94°, that of chabasite P on P, 94° 24′. Phillips makes the last 94° 46′. The analyses of Anderson and Rammelsberg would seem, at first, to shew a marked difference in their composition, a difference which is also shewn by the different analyses of common chabasite, resulting in varieties having different formularic expressions. For example, Acadiolite contains three per cent. more of silicic acid than common Chabasite, and is a tersilicate of lime and the other isomorphous bases, instead of a bisilicate of the same bases. The mineralogical formula of Acadiolite is, $3\ \mathrm{Al}\ \mathrm{Si}^2 + (\mathrm{Cal}, \mathrm{N}, \mathrm{K}), \mathrm{Si}^3 + 6\ \mathrm{Aq.}$, while that of

Chabasite is, $3\ \mathrm{Al}\ \mathrm{Si}^2 + (\mathrm{Cal}, \mathrm{N}, \mathrm{K}),\ \mathrm{Si}^2 + 6\ \mathrm{Aq}$. Rammelsberg is inclined to regard Phacolite as a mixture of Acadiolite and Scolecite (lime mesotype), the latter containing an additional atom of water.* By uniting the atoms of both, he thus states the chemical formula for Phacolite: $2\ \dot{\mathrm{R}}\ \dddot{\mathrm{Si}} + \dddot{\mathrm{Al}}^2\ \dddot{\mathrm{Si}}^3 + 10\ \dot{\mathrm{H}}$. As the analyses stand (compare Berzelius's and Thomson's with the two just referred to), Phacolite differs from Chabasite in containing three per cent. less of silicic acid, and three atoms less of water. Now it is obvious, that these differences are insufficient to authorise a separation of the two minerals, unless there be a want of agreement in crystallographical and other characters, greater than that as yet pointed out. An equally valid reason could be urged for the separation of Acadiolite from Chabasite, on the ground of a difference in their composition, had not the examinations of Professor G. Rose proved an exact agreement in the angles of their primary crystals. So also of Levyne and Gmelinite, which are now admitted to be only varieties of Chabasite, their occurring forms all being secondaries to the same primary rhombohedron. The evidence of the identity of any two minerals is best shewn by the incipient or intermediate passages of one into the other, in the same specimen. I am not aware that, in the case of the Irish or Bohemian Phacolite, such evidence has been adduced; no tendency of the sort is shewn in the specimens I have examined from those countries. Now, one of my specimens from New York has the distinct form of Chabasite (the perfect rhombohedron), and of Phacolite (perfect double six-sided pyramids). The first form, however, is rare; the incipient replacements are also shewn; but these crystals have not the full perfection of waxy lustre reflected by the ultimate form of Phacolite,—a singular effect, attributable, probably, to the nature of the solvent in which the molecules were suspended.

Approach of twin-crystals to the Phacolite form.—These, as they are sometimes presented, would, unless carefully examined, be mistaken for the true form of Phacolite. The most perfect specimens I have seen, are from Nova Scotia. They consist of two rhombohedrons united in the usual manner, each crystal turned half round, but having their superior edges and lateral angles deeply replaced. The approach to the form of Phacolite is thus produced; the edges and angles not standing out in relief, as they ordinarily do in these twin forms. The striæ, parallel with the edges of the two rhombohedrons, so intersect as to shew the compound nature of the crystals. Dr C. T. Jackson has a fine specimen of this variety from the Two Islands, in Nova Scotia, of a wine-yellow colour; I have another, pure white, from the same place.

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* See First Suppl. to his *Handwörterbuch*, p. 112. It was on these grounds that Hoffman proposed to separate acadiolite, as well as the Gustafsberg variety, from chabasite.—*Poggendorf's Annalen*, xxv., 495.

is difficult to obtain the mineral entirely free. Its infusibility before the blowpipe would seem to shew this. It has been suggested, also, that a part of the iron may have been in the state of protoxide. It seems impossible, without some such supposition, that substances so closely resembling each other in all their physical characters, should differ so much in chemical composition. Now, if the potash be left out, and the peroxide of iron be changed into protoxide, the ratio between the atoms of acid and bases is nearly the same as in Ottrelite, if we unite the atoms of magnesia and iron, as isomorphous with each other. Ottrelite, also, is not easily separated from its matrix, but the larger size of its plates would seem to render it more easy to obtain pure specimens for analysis; and it is to be observed that Damour repeated his analysis, and obtained precisely the same result. It is remarkable that Rammelsberg has alphabetically inserted Phyllite, but has given no formula for its constitution. It seems proper that the name of Phyllite, on the ground of its priority, and because it expresses so well the ordinary appearance of the mineral, should stand, and that of Ottrelite be abandoned.†

Dysluite identical with Automalite. I am satisfied, from recent observations, that these two minerals, as they occur in New Jersey, should form but one species. The difference in hardness, colour, specific gravity, and pyrognostic characters, can be accounted for by the well established fact of the isomorphous replacement among the constituents of certain minerals which do not differ in crystalline form. In Dysluite, we have but thirty per cent. of alumina, the acting acid principle in the mineral, while, in Automalite, we have sixty per cent. But the peroxide of iron, which is isomorphous with the alumina, amounts to nearly forty-two per cent Now, if we suppose about thirty per cent., of this peroxide of iron, to have replaced the same number of atoms of alumina in Automalite, and the eight per cent. of protoxide of manganese to have replaced so much of the oxide of zinc, we make up, very nearly, the essential constituents, as shewn in the analysis of Automalite by Ekeberg and Abich. It is to be observed that the latter chemist puts down the iron as *protoxide* in the Franklin Automalite. If it should prove that the iron exists in Dysluite in both states of oxidation, the twelve per cent., remaining out of the forty-two, may be protoxide, replacing so much oxide of zinc. So that, in this view of the case, the 17 per cent. oxide of zinc + 11 per cent. protoxide of iron + 7 per cent. protoxide of manganese = 35 per cent. oxide of zinc, which is nearly the exact quantity found by Abich in the crystals from Franklin. We may then state the constituents as follow :—

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† Brooke has supposed Phyllite to be identical with Gigantolite. If we compare the analysis of Gigantolite with Damour's analysis above, the evidence of their identity (supposing Ottrelite to be a purer variety of Phyllite) is much more marked, and the ratio between the atoms of acid and bases is nearly the same in each.

		Oxygen.		Ratio.
Alumina,	30·49	14·24	23·43	3
Peroxide of iron,	30·00	9·19		
Protoxide of iron,	11·93	2·72	7·76	1
Protoxide of manganese,	7·60	1·70		
Oxide of zinc,	16·80	3·34		

Here it is evident that the atoms of acid and bases are to each other as three to one, which is the case, also, with Automalite, taking Abich's analysis, and grouping the isomorphous bases, thus:

		Oxygen.		Ratio.
Alumina,	57·09	26·66		3
Oxide of zinc,	34·80	6·92	8·72	1
Magnesia,	2·22	·76		
Protoxide of iron,	4·55	1·04		

Dr Thomson, the only chemist who has analysed Dysluite, reckons all the iron as peroxide, and as the principal basic constituent of the mineral, which, in his view, consists of the aluminates of iron, zinc, and manganese. Rammelsberg, in stating the analysis, has given both oxides; and the atoms of alumina and peroxide of iron, as put down by him, are 22·80, and those of the isomorphous bases—protoxide of iron, protoxide of manganese, and oxide of zinc—are 7·83 (7.89 ?); thus giving the same ratio as that above stated.

But other reasons may be urged why Dysluite should be regarded only as a variety of Automalite. I have seen specimens on which there were crystals well claiming the name of Dysluite, as well as others equally entitled to the name of Automalite; while there were yet others, evidently passing from one into the other,—the bright and perfect crystals of Automalite gradually losing their lustre, becoming porous, comparatively brittle and soft. I think if these circumstances had been attended to in the early history of the mineral, the name Dysluite would long since have departed from the catalogue of mineral species.

Polyadelphite.—As Dana, in the new edition of his Mineralogy, has very properly included this mineral under the species garnet, I merely refer to it, to give further evidence of the correctness of his opinion, from circumstances connected with its occurrence at the locality. It is evidently a granular, imperfectly crystallized yellow garnet, and the specimen which I received, ten years ago, from Professor Nuttall, contains mechanical mixtures which it would be impossible to separate from it, so as to give us entire confidence in its analysis. To these, I believe, we may attribute its departure in composition from the common brown or yellow garnet, though it does not differ much from the brown garnet of Franklin, analysed both by Dr Thomson and Mr Seybert.

Beaumontite of Levy, and *Lincolnite* of Hitchcock.—In a paper read before the Boston Society of Natural History, and since published in their Transactions, and in the American Journal of Science (vol. xlvi., p. 235), I gave my reasons for classing these two minerals with Heulandite. That Beaumontite is Heulandite, I believe, is no longer doubted in this country or Europe. An analysis of the mineral by M. Delesse, has appeared since the publication of my paper,* and it agrees with all the other analyses of Heulandite, excepting in the slight excess of silicic acid. In this respect, it offers an example analogous to that of the variety of Chabasite, called Acadiolite, in which the silicic acid forms a larger atomic proportion of the mineral, without causing any appreciable variation in the angles of the crystals. As to Lincolnite, I must think that the various papers, that have been called forth in relation to it, since my first communication appeared, have established its indisputable identity with Heulandite.

Peculiarities in the modifying planes,† have given rise to a secondary form, rarely observed in Heulandite. These consist in the enlargement of the planes f (Phillips), or $\breve{e}$ (Dana), so as nearly to obliterate the primary planes M; being, in fact, the reverse of what we usually observe in Heulandite from other localities. In the measurements by Professor Hitchcock and Professor Shepard, the angle of f on T was mistaken for that of M on T, and in the figure given by Professor Hitchcock, it is evident that the planes lettered M should be f. The true value of f on T is 115° 10′ (Dana); Professor Shepard's last measurements made it 116° 17.′

Ledererite.—I am compelled at last to declare my conviction, that the specific nature of this mineral can no longer be maintained. Connell's analysis of an Irish Gmelinite, which agrees with Ledererite in all its physical and crystallographical characters, has shewn also an identity in chemical composition. The phosphoric acid, detected by Mr Hayes, must be viewed as an accidental constituent, varying probably in different crystals, or, in some of them, not existing at all. Some of the Zeolites, in the Nova Scotia trap, have been found associated with small crystals of phosphate of lime, and it is not impossible that some of the minutest of these may have intercrystallized with the Ledererite. We regret that we have not been able to obtain other specimens to enable Mr Hayes to give it a re-examination. For comparison, I subjoin the analyses of Ledererite and Gmelinite.

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* *Ann. de Chim. et de Phys.* for 1843, t. ix., p. 395. Phillips's Min., p. 627.

† For the figures see Am. Jour. of Science, vol. xlvi., p. 234, and vol. xlvii., p. 416. Corroborative evidence of the correctness of my opinions, by the editors of the Am. Jour. of Science, may be seen at the pages here referred to.

	Ledererite.		Gmelinite.
Silica, . . .	49·47		48·56
Alumina, . .	21·48		18·05
Lime, . . .	11·48		6·13
Soda, . . .	3·94		3·85
Phosphoric Acid,	3·48	Potash,	0·39
Protoxide of Iron,	0·14		0·11
Water, . . .	8·58		21·66
	98.56, Hayes.		98·75, Connell.

Now, if the phosphoric acid in Ledererite is united with lime as an accidental mixture, 2½ per cent. of the lime should be taken from the 11·48 per cent. found in the mineral: this brings the proportion down nearly to that obtained by Connell. Mr Hayes was not able to determine the weight of the water with accuracy, owing to the small quantity of the mineral operated upon. As the loss (1·44 per cent.) was mostly water, we may suppose, with Rammelsberg, that Ledererite is Gmelinite containing ⅓ (½ ?) its quantity of water. The chemical formula for Gmelinite and Chabasite is thus :—

$$(\dot{Ca}, \dot{Na}, \dot{K}^3)\, \dddot{Si}^2 + 3\, \dddot{\bar{Al}}\, \dddot{Si}^2 + 18\, \dot{\bar{H}}.^*$$

Excepting the absence of striæ, and the shorter dimensions of the prismatic planes of its crystals, the Irish Gmelinite precisely resembles Ledererite; their hardness, lustre, colour, and blowpipe characters, are the same. The appearance of hexahedral cleavage, on which Dr Jackson originally founded the chief claim of the latter to the character of a new species, was only imperfectly produced by heating the crystals, and not by ordinary mechanical cleavage. This could not be effected, the mineral breaking, in all directions, with a vitreous fracture. Dr Jackson agrees with me, that it can no longer be retained as a distinct species.

While preparing my edition of Phillips's Mineralogy, I requested Mr Hayes and Dr Jackson to make several analyses for me with particular reference to that work. As some of these have not appeared in any other form, I wish now to make a permanent record of them, in order that they may be seen where they might not otherwise reach, the first are of the Nova Scotia Chabasite (Acadiolite), which Hoffmann has distinguished from common Chabasite, by its containing 3 per cent. more silica, and for which Rammelsberg has given a formula differing somewhat from that of Chabasite. (See first part of this article).

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* *Handwörterbuch*, i., 150. Rammelsberg unites Chabasite and Gmelinite, the first as soda Chabasite, the last as lime Chabasite. This is in accordance with Tamnau, who has established their identity on crystallographical grounds. The close relation of the two minerals was, however, first shewn by Prof. Mohs. See his *Mineralogy*, vol. ii., p. 105.

Silica,	52·02	52·20
Alumina,	17·88	18·27
Lime,	4·24	6·58
Potash,	3·03	2·12
Soda,	4·07	
Water,	18·30	20·52
	99·60, Hayes.	99·69, Hayes.

These results agree with those obtained by Hoffmann,[*] in his analysis of the same mineral, the specimens of which were presented to him by Charles Cramer, Esq., of St Petersburg.

Washingtonite of Shepard, analysed by Mr J. S. Kendall, under the direction of Dr Jackson, gave these results :—

		Oxygen.	Ratio.
Titanic acid	25·28	4·82	1
Peroxide of iron,	51·84	10.36	2
Protoxide of iron,	22·86	5·08	1
	99·98		

The atomic proportions are thus nearly one atom titanic acid, two atoms peroxide of iron, one atom protoxide of iron ; or a tritianiate of iron, consisting of two atoms tritianiated peroxide, and one atom trititaniated protoxide. If we unite the magnesia and lime with protoxide of iron, in the following analysis of an Ilmenite from Arendal,[†] by Mosander, we obtain precisely the same result. The crystalline form of the two varieties is also the same, and there can be no doubt of their identity as one species.[‡]

Titanic acid,		24·19
Peroxide of iron,		53·01
Protoxide of iron,	19·91	20·92
Magnesia and lime,	1·01	

By referring to the analyses of Ilmenite from other localities, it will be seen that the essential constituents, titanic acid and the two oxides of iron, so interchange with each as to produce different varieties, but all having the same crystalline form.[*]

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[*] American Journal of Science, vol. xxx., p. 366.

[†] The Hystatite of Breithaupt.

[‡] An acute rhombohedron, P on P 86° 10′, for the Ilmenite. Shepard, employing varnished planes of the Washingtonite, makes P on P 86°. Professor Shepard founds the distinction on other than crystallographical characters ; for, he says, it is not thus " shewn to be distinct, in any essential manner, from the axotomous iron-ore of Mohs, or from Crichtonite (including Ilmenite) ; indeed, it appears most probable, that all these minerals are not only identical in their angles, but are ismorphous with specular iron."—*American Journal*, vol. xliii., p. 365. The analysis, now, would seem to destroy the groundwork for any distinction.

An Experimental Investigation of the Magnetic Characters of Simple Metals, Metallic Alloys, and Metallic Salts.[†] By WILLIAM STURGEON, Esq., Lecturer on Natural and Experimental Science, formerly Lecturer on Experimental Philosophy at the Honourable East India Company's Military Academy, Addiscombe.

1. The subject to which this memoir is directed is one that has been already explored at different periods by many eminent experimental philosophers, who have recorded the results of their labours in various works on science; and as several interesting and well-attested facts have been discovered by those investigations, but little would appear to be wanting at this time to accomplish all the desirable information respecting the magnetic actions of bodies, whether of a ferruginous or a non-ferruginous character. Under these circumstances, any further investigations of mine or of any other experimental inquirer could be productive of nothing more than the development of a few novel facts in addition to those already recorded, or the application of some of them to novel and useful purposes.

2. It frequently happens, however, in experimental inquiries of this kind, that the different modes of investigation resorted to by different philosophers, are not only productive of new facts, but are the means of developing new laws, and of leading to theoretical views differing considerably from those previously entertained for the explanation of phenomena which had long been grafted into the history of science. And it must be acknowledged that, whether new facts be added to the old stock—the application of any of them to

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[*] Journal of the Boston Society of Natural History.

[†] Read April 7, 1846, to the Philosophical Society of Manchester. From Memoirs of Society.

useful purposes discovered—or that novel and more exact views for the explanation of those facts be developed, an additional step in the advancement of science would thus be securely established; and it is solely from a hope that by *some* of these means the present memoir will contribute to the progress of scientific knowledge, that I have ventured to offer it to the consideration of this Society.

3. The superlative degree of magnetic action displayed by metallic iron, above that of all other known bodies, has been a theme of almost continuous contemplation and philosophical speculation from remote periods in the history of science till the present day, and continues to be a subject of interest and admiration throughout every part of the scientific world. The ordinary laws of the magnetic action of metallic iron, especially when in masses, are, however, now so satisfactorily established, and the phenomena so well known, that any further notice of them in this place would be foreign to the object of this memoir; more especially as it alludes to the simple magnetic attractions only, whilst illustrating the novel facts it contains, and the mode by which they were developed.

4. Nickel is a metallic body, which, next to iron, stands most distinguished for the display of magnetic action: and indeed, notwithstanding the number of inquiries that have been made respecting the magnetic action of other bodies, and the talent and expedients that have been employed in the pursuit, little or nothing has been satisfactorily ascertained beyond that which is so conspicuously displayed by those two metals,—iron and nickel. (7–13.)

5. There is something very remarkable, however, respecting the magnetism of these two distinguished metals when in combination with other bodies. Nickel, for instance, is said to lose all its magnetic action when combined with even a small dose of arsenic, and iron has long been understood to suffer the same fate when alloyed with antimony. Beyond these two alloys of nickel and iron, I am not aware that any other have been magnetically investigated, although, as will appear in the sequel, some of the most extraordinary facts that have hitherto appeared in the magnetism of me-

tallic bodies, are displayed by alloys of iron with other metals.*

6. Tiberius Cavallo was amongst the earliest inquirers into the magnetic action of non-ferruginous metals; but the principal part of his experiments were limited to copper and brass, specimens of both of which he found to be magnetic; and especially after they had suffered the operation of hammering. The investigating apparatus of this philosopher, like that of many subsequent inquirers, consisted of a delicate magnetic needle, to the poles of which the specimens under examination, to prevent commotion in the air, were slowly and dexterously presented.†

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* At the time the first part of this memoir was read, I was not aware of these curious and interesting facts. They were subsequently discovered, and are described in the second part.

† The following are the conclusions at which Cavallo arrived respecting the magnetism of brass:—

"*1st,* Most brass becomes magnetic by hammering, and loses its magnetism by annealing or softening in the fire, or, at least, its magnetism is so far weakened by it, as afterwards to be only discoverable when set to float in quicksilver.

"*2d,* The acquired magnetism is not owing to particles of iron or steel imparted to the brass by the tools employed, or naturally mixed with the brass.

"*3d,* Those pieces of brass which have that property, retain it without any diminution after a great number of repeated trials, viz., after having been repeatedly hardened and softened.

"*4th,* A large piece of brass has generally a magnetic power somewhat stronger than a smaller piece, and the flat surface of the piece draws the needle more forcibly than the edge or corners of it.

"*5th,* If only one end of a large piece of brass be hammered, then that end alone will disturb the magnetic needle, and not the rest.

"*6th,* The magnetic power which brass acquires by hammering has a certain limit, beyond which it cannot be increased by further hammering. This limit is various in pieces of brass of different thicknesses, and likewise of different qualities.

"*7th,* Though there are some pieces of brass which have not the power of being rendered magnetic by hammering, yet all the pieces of magnetic brass that I have tried lose their magnetism, so as no longer to affect the magnetic needle, by being made red hot, excepting, indeed, when some pieces of iron are concealed in them, which sometimes occurs; but in this case the piece of brass, after having been made red hot and cooled, will attract the needle more forcibly with one part of its surface than with the rest of it; and hence, by turning the piece of brass about, and presenting every part of it successively to the

7. As brass in an alloy so extensively employed in the construction of magnetic compass-boxes, its magnetic or non-magnetic condition is an important scientific inquiry, which, though for many years in the hands of philosophers, remains at this day as undetermined as when first undertaken. That certain pieces of brass have displayed unequivocal magnetic action is a fact which cannot be questioned, but whether that action was due to the alloy of pure copper and zinc alone, or to portions of iron accidentally present in the metal, different opinions have been entertained.*

8. It has been supposed by Cavallo and other philosophers, that all bodies, whether metallic or otherwise, are endowed with magnetic powers, which vary considerably in degrees of energy, whilst under the influence of, or operating on, the magnetic needle. But when we meet with such conflicting opinions as those that appear in the writings of philosophers so eminent in this department of physics as Cavallo, Coulomb, Bennet, Haüy, Biot, Becquerel, and others who have entered

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suspended magnetic needle, one may easily discover in what part of it the iron is lodged.

"8*th*, In the course of my experiments on the magnetism of brass, I have twice observed the following remarkable circumstance:—A piece of brass, which had the property of becoming magnetic by hammering and of losing the magnetism by softening, having been left in the fire till it was partially melted, I found upon trial that it had lost the property of becoming magnetic by hammering; but having been afterwards fairly fused in a crucible, it thereby acquired the property it had originally, viz., that of becoming magnetic by hammering.

"9*th*, I have likewise often observed, that a long continuance of a fire so strong as to be little short of melting hot, generally diminishes, and sometimes quite destroys, the property of becoming magnetic in brass. At the same time the texture of the metal is considerably altered, becoming what some workmen call *rotten*. From this it appears, that the property of becoming magnetic in brass by hammering is rather owing to some particular configuration of its parts than to the admixture of any iron; which is confirmed still further by observing that Dutch plate brass (which is made, not by melting the copper, but by keeping it in a strong degree of heat whilst surrounded by *lapis calaminaries*) also possesses that property."

* During the interesting series of experiments carried on by Professor P. Barlow, on the magnetism of ferruginous bodies, the brass compass-box, and several brass screws, belonging to one of the finest looking instruments employed, were found by that philosopher to be higly magnetic.—*Barlow's Magnetic Attractions*, Second Edition, p. 17.)

this field of research, we are necessarily led to infer that the subject has not yet been accurately and satisfactorily determined.

9. The beautiful experiments of M. Arago, and the final development of magnetic electricity by Dr Faraday, afford an ample explanation of nearly all those experiments in which vibrations of the magnetic needle, near the bodies under examination, were taken as evidence of their magnetic actions; as well as in all those cases in which light needles of the bodies examined were vibrated under the influence of powerful magnets. It is reasonable to suppose, also, that thermo-electric currents would influence the results of those experiments which were made previous to the discovery of that branch of electricity by Dr Seebeck, especially in those cases in which the bodies under examination were held in the hand whilst presented to the magnetic needle.

10. There are, however, some phenomena on record, the explanations of which do not appear to fall within the range of the laws either of magnetic-electricity or thermo-electricity; and, therefore, the cause of their development is necessarily located in some other source. For instance, when Coulomb employed light, delicately suspended needles of gold, silver, glass, wood, and other substances, both organic and inorganic, he found them obey the polar forces of a magnet in precisely the same manner as needles of iron would do; for, after the vibrations had ceased, those needles became arranged between the north and south poles of powerful magnets, in such manner, that their axis rested in the line of magnetic force, or in a right line joining the magnetic poles employed.

11. It is somewhat remarkable that, when similar experiments were made by M. Becquerel, the results were very different. By employing needles of wood, lac, and some other substances, this philosopher found that the positions they assumed when at rest directly between the north and south poles of powerful magnets, were invariably at right angles to a right line joining those poles; and, consequently, at right angles relative to the magnetic forces, or to the position in which the needles of Coulomb rested. From the

results afforded by the experiments of M. Becquerel, that philosopher has been led to consider that the effects produced by a strong magnet on a magnetic needle, or on soft iron, differ essentially from those which take place in all bodies whose original magnetism is very weak. In the former the magnetic axis of each is arranged in its *length;* but in the latter class of bodies the magnetic axis becomes arranged transversely. M. Becquerel shews, however, that wooden needles assume different positions, with respect to the magnetic poles, according to the distance at which they are placed from them.*

12. In a paper by Dr Faraday, read before the Royal Society of London in January last, it is stated, that a variety of bodies, bismuth being the most eminent in this respect, arrange themselves with regard to powerful magnetic poles, in precisely the same manner as described by Becquerel; that is, with their longest axis at right angles to the line of magnetic force.

13. Dr Faraday has attempted a classification of a great number of bodies under the two following heads :—*Magnetics* and *Diamagnetics*. The former class, of which iron is the grand type, become arranged, whilst under magnetic influence, with their longest axis in the magnetic line of force; and the latter class, of which bismuth is the type, become arranged at right angles to the magnetic line of force. In the magnetic class, Dr Faraday places some of those bodies which, according to M. Becquerel's nomenclature, would be placed in the other class, or amongst those which become arranged at right angles to the line of magnetic force. Such, however, is the condition of this interesting inquiry at the present time, no two of those hitherto engaged in it having arrived at similar results in any series of experiments that have been undertaken.

14. The inquiries that I have made in this department of magnetics have been conducted partly by the employment of magnetic needles, partly by permanent steel magnets, and

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* Traité Experimental de l'Electricité et du Magnetisme, tome ii., p. 387.

partly by electro-magnets, which have afforded different modes of assailing those substances that were the objects of investigation.

15. In all cases where delicate magnetic needles are employed, especially when the suspension is by means of a fibre, and the system astatic, the experiments are exceedingly tedious, and much time is required to allow of the system's repose from its agitations before an attempt can be made to approach it with the specimen to be examined. When, however, a single needle is employed, whose support is a finely pointed pivot, the experiments are less subject to delay than by the other mode, though much caution and some dexterity are still required to enable the experimenter to arrive at satisfactory results. But in whichever way the magnetic needle may be suspended in these delicate investigations, the bodies under examination must either be held immediately in the hand, or indirectly, by means of some other body previously ascertained to have no influence on the needle. If held in the hand, thermo-electric currents are to be suspected; and if attached to the end of a wooden rod, by means of sealing wax, or resinous cement, other electric actions may interfere with the results; or may, indeed, be the sole cause of any motions that may happen to be observed. Moreover, a delicate magnetic needle does not possess a sufficient degree of power to bring into play the minute portions of magnetism that lie dormant in many bodies. These exiguous sleeping forces can never be roused into a state of activity, and, consequently, can never be discovered by merely presenting the bodies in which they reside to the pole of a feeble magnetic needle. To accomplish their discovery a comparatively powerful magnetic action is absolutely required; for, when thus assailed, their polarization is more easily enforced, and their detection almost certain. The magnetic needle, however, may be usefully employed in cases where the suspected magnetism of a body is of some easily detected amount; and it may be resorted to with advantage, in preliminary trials, under all circumstances, because of the possibility of the specimen under examination possessing a sufficient amount of magnetism to be detected by it, and the

more tedious modes of inquiry being thus rendered unnecessary.

16. I have found that a convenient and efficacious mode of examining bodies the magnetic actions of which are very very feeble, and others in which magnetism has but a questionable existence, is by means of an apparatus represented by the accompanying figures.

Fig. 1.

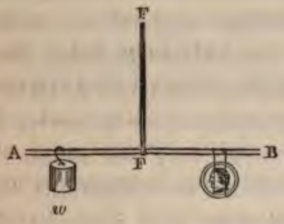


Fig. 2.

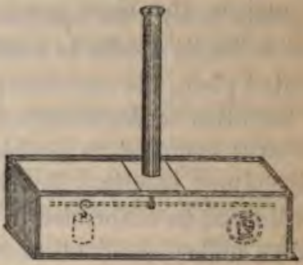
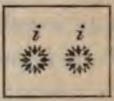


Fig. 1 is that part of the apparatus in which the specimens to be examined are placed. It consists of a light cylindrical wooden rod A B, about 12 inches long, and suspended by a few parallel fibres of silk F F, from the cocoon. The end B is furnished with a light slip of card-paper and two loops of horse-hair, for the purpose of holding the specimen; say a half-crown, for instance, as represented in the figure, which is counterbalanced at the other end of the lever by a sliding weight *w*. This part of the apparatus is enclosed in a rectangular box, Fig. 2, whose ends, top, and one of its sides, are of glass, and a brass tube rises from the middle of the top, in which hangs the silken fibres. The head of this tube sustains the fibres and their appendages, and can be turned in any horizontal direction for the adjustment of the lever to a parallelism with the sides of the box.

The glass parts of the box are sustained by a light mahogany frame, with a bottom of the same kind of wood. The ends and sides are fixed, but the top, which consists of two sliding parts, can be removed at pleasure, for the purpose of introducing the hands for the adjustment of the apparatus within, and replaced when the specimen has been accurately counterpoised, a process which is still farther facilitated by the introduction of the hand at one side of the box, which is opened for that purpose, and afterwards closed by a sliding mahogany door.

When the agitations of the lever have subsided, the sliding door is partially opened for the introduction of the poles of a powerful steel horse-shoe magnet, which is made of a long and narrow shape for the purpose. This magnet is placed on a sliding carriage, by means of which it is made to approach the specimen, or recede from it, with great facility, and in the most gentle manner. In consequence of finding decided polarity in some specimens in which no magnetism was previously known to exist, I have been led to the employment of a powerful bar-magnet, which I find convenient in those cases where such polarity is suspected. I have also employed electro-magnets, both straight and of the horse-shoe form; but having found that these are troublesome and inconvenient, I have abandoned the use of them altogether in these inquiries.



In cases where extreme nicety is required, I have found the following mode exceedingly useful. Besides the sliding door which closes one side of the box, I have another sliding piece P, which fits into the same grooves in the side of the box. Through this piece pass two cylindrical rods of soft iron, *i i*, about 2 inches in length. They are firmly fixed in the wooden slider at their middle parts, and parallel to each other. When this piece is in its place, the iron rods are in the same horizontal plane, having one half within and the other half outside the box, and their inner ends presented to the specimen suspended on the lever. When the remaining portion of that side of the box is closed by the sliding door, the specimen is nicely adjusted to the ends of the iron rods by turning the top piece of the tube, until the most trifling space is perceptible between them. When all is at rest, the poles of the horse-shoe magnet are made to approach the outer ends of the iron rods, bring them into magnetic action, and thus detect the magnetism of the specimen, if any exist in its structure.

17. By the assistance of this apparatus, to which I give the name *Torsion Magnetoscope*, I have examined gold, silver,

copper, platinum, tin, antimony, lead, zinc, bismuth, and mercury; and also some of their alloys and salts. In none of these metals, when in a state of purity, have I been able to discover the slightest trace of magnetic action, though in several specimens of some of them, as they appear in a commercial state, magnetic action is strongly developed.

18. A bar of bismuth, for instance, cast from a mass fused in an earthenware crucible, was found to be highly magnetic, and, for a while, was considered as a good specimen of the magnetic action of that metal; but on examining another bar cast from the remaining portion in the crucible, and finding it still more powerfully magnetic than the former, a suspicion was aroused that either their crystalline structures were different to each other, or that the metal was not pure. The experimental inquiries which this suspicion occasioned, led to the detection of localities in the two bars in which the magnetic actions were more powerful than in the other parts of them, which gave rise to the determination of *sweating* one of the bars at a low heat, and running out of the crucible the most easily fused portions, before the rest became fluid, which is an excellent process for freeing pure bismuth from some of the impurities with which it is frequently contaminated in the mercantile state.

19. This purified bismuth having been cast into a bar, was afterwards broken into convenient fragments, and tested by the *Torsion Magnetoscope*, previously described (16); but not the slightest trace of magnetism could be detected in any of the pieces.

20. Having satisfied myself that no magnetic action resided in the pure bismuth, the dross left in the crucible was softened by heat, and poured on a stone slab, and on being tested developed highly magnetic powers. It now became obvious that the whole of the magnetism displayed by the bismuth, when in its first state (17), was due to that portion only which was left as dross in the crucible after the pure metal had been run out.

21. From the results thus arrived at, I was induced to fuse other portions of mercantile bismuth, and run out the purest portions of the metal at the lowest degree of fusible heat, by

which means I have been enabled to separate the magnetic from the unmagnetic portions, and thus to arrive at the conclusion, that pure bismuth is not susceptible of any *direct* magnetic action by the mere approach of the poles of a powerful magnet, which I consider a test of far greater certainty and exactness than that of a feeble magnetic needle, whatever may be the delicacy of its suspension; and as a peculiar class of phenomena become displayed by a sudden development of the powers of an electro-magnet, much misconception might rise from its employment.

22. The most usual impurities of bismuth of commerce are sulphur and arsenic, and occasionally a small portion of silver and iron. On subjecting the drossy part (19) to dilute sulphuric acid, a portion was dissolved; after which the liquid was reduced almost to dryness by evaporation. The residue being diluted with water, and a solution of ferrocyanuret of potassium being added, it assumed a blue colour, which indicated that a portion of iron had been dissolved from the mass. On dissolving another portion of the dross (19) in dilute sulphuric acid, the presence of iron was again indicated by the addition of bruised gall-nut. Hence it was fair to infer, that the whole of the magnetic action displayed by the mass was due to the iron it contained.

23. I have examined antimony in the same way, and have found some specimens magnetic, and many others in which magnetic action could not be detected. By thus operating on antimony, however, it would be impossible to form a correct idea of the magnetic or unmagnetic state of that metal; because, as will appear in the sequel, of its masking, to a considerable extent, the magnetism of even considerable proportions of iron, when the two metals form a perfect alloy. Hence, in order to test antimony magnetically, it becomes necessary to ascertain, by chemical processes, that it is perfectly pure, and especially that it is free from iron; for, although antimony will mask the magnetism of iron when in perfect union with that metal, a very trifling proportion of uncombined iron will render the whole mass *apparently* magnetic. By attending to these particulars, pure antimony will not be found to display any magnetic action. (17.)

24. It is generally understood, principally, I believe, upon the authority of Dr Seebeck, of Berlin, that iron becomes "completely destitute of magnetic action" when alloyed with four times its weight of antimony.* This, however, does not appear to be correct, for I have formed very perfect alloys of these two metals, in a great variety of proportions, and find that when the iron does not form even a twentieth part of the mass, it is still magnetic, though in a very low degree. When the alloy is of equal parts of iron and antimony, it is highly magnetic. This alloy, when broken, exhibits a dark grey fracture, somewhat glittering. It is easily reduced into powder by the operation of a file, or by pounding in a mortar; and what is very remarkable, it yields an abundance of deep crimson sparks when struck against hard steel.

25. It has already been stated (17), that pure copper is not magnetic; and I must now add that, in a very few cases only have I detected magnetic action in the copper of commerce, although I have tested a great number of specimens both in the state of sheet and of wire.

26. In the copper coinage of this country, I have never yet met with magnetic action, notwithstanding the number of experiments I have made on the various copper coins that have been struck in the reigns of several sovereigns.

27. In the gold and silver coinage, however, in which copper forms a constituent part, the case is very different. These alloys are nearly all of them decidedly magnetic, and, probably, none of them entirely free from magnetism. The gold coinage, however, displays much feebler magnetic action than the silver coinage; indeed, in many gold coins the existence of magnetism may be considered as questionable, whilst in others, and especially in those of 1844, magnetic action is prominently displayed.

28. The silver coinage, although in some specimens scarcely any magnetic action can be detected, is generally magnetic in a very eminent degree; and I have found that, when any one piece of a particular coinage displays considerable magnetic action, the whole of that coinage, as far as I have ex-

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* Brewster's Magnetism, p. 102.

amined it, is similarly magnetic. And, on the other hand, when a silver coin has been found to be but very slightly magnetic, I have but rarely met with one of that particular coinage in which any considerable degree of magnetism could be detected.

29. Of the silver coins that have come under my notice, a half-crown of William and Mary, dated 1691, is the most eminently magnetic. The next, in point of magnetic action, are the half-crowns of 1844 and 1845; then one of George IV., the date of which I have not noted. The half-crowns of George III., of 1819 and 1820, are more slightly magnetic than those last named, and the half-crowns of both coinages in 1817 are still less magnetic than those of 1819 and 1820. Shillings, also, of certain coinages, are magnetic in an eminent degree, and there are but few, if any, that I have examined, that are entirely neutral to the high magnetic powers with which they have been assailed.

30. Silver articles for domestic purposes, such as spoons, prongs, fruit-knives, &c., were, in many specimens, found to be much more magnetic than any of the silver coins that I have examined. I have borrowed several sets of silver tea-spoons from neighbouring families, and, with the exception of one half-dozen of Scotch spoons, of a very old date, all have displayed high magnetic powers, though of very different degrees in different sets. But what is very remarkable, if one individual spoon was found to be highly magnetic, the whole of that particular set, whether it consisted of half a dozen or a dozen spoons, were highly magnetic also. And, generally, whatever might be the magnetic condition of any individual spoon, the whole number of the set to which that spoon belonged were magnetic alike, or very nearly so. Hence, if the quantity of magnetic action of any individual spoon were to be denoted by q, and the number of spoons in the set denoted by n, the sum-total of magnetic action in that set of spoons would be $n\,q$ nearly. Of course, this reasoning applies only to individual sets of spoons which are of uniform make, composition, and structure of metal. It appears also, as far as my experience has extended, that the same mode of reasoning

would give the sum-total of all the magnetic action that any individual coinage would display. Suppose, for instance, the magnetism displayed by a half-crown piece were to be taken as the unit of quantity equal q, then the number of the pieces being n, the sum-total of magnetism which the whole of that coinage would display would be $n\ q$ nearly; and similarly for any other coinage of silver.

31. The difference of magnetic action displayed in the silver coinage and domestic articles of that metal (29, 30), led to the supposition that minute portions of iron might accidentally have got introduced to the alloys whilst in a state of fusion, which had some probability in its favour, from the fact that the metal for silver coinage is fused in cast-iron pots,* and, therefore, liable to take up a portion of those vessels. But, on the other hand, if that were always the practice, it would lead to the inference that in all the silver coins the iron would be nearly in the same proportion, and the extent of magnetic action almost the same in all; whereas, by the tests already described, this is not the case.

32. The current silver coinage of William and Mary became so base, that in the year 1694 it was all called in, and a new coinage issued. From this fact, it occurred to me that there was a possibility, at least, that the high degree of magnetic action displayed by the half-crown of 1691 (29), was owing to an undue proportion of copper, or of some other inferior metal. This idea led to the selection of a shilling, in which scarcely a trace of magnetism could be detected, for fusion with an additional portion of copper, also non-magnetic, having been obtained by the electrotype process. These, together with a piece of pure silver, were fused in an earthenware crucible, and run out upon a sheet of copper. The copper in this alloy was about one to five of silver, which is more than twice the proportion of that in the standard coinage. On subjecting this mass to the *torsion magnetoscope*, it was

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* Ure's Dictionary of Arts and Manufactures. Brande's Chemistry, page 1073.

found to be more higly magnetic than the old half-crown of William and Mary (29.)

33. This singular result has cost me much thought and a great deal of trouble. The crucible employed was quite clean, having never been used before; and its contents during the time it was in the fire were the silver and copper, and a mixture of pulverized charcoal and common salt. Similar pieces of charcoal and slices of the same quality of salt have been tested, but no magnetic action could be detected in either. Whence, then, this almost unexpected magnetism in the metallic alloy? Fragments of a broken crucible similar to that used were found to be slightly magnetic, probably from a portion of iron in the clay of which it was formed: but the magnetic action in the fragment of the crucible was not nearly so great as that displayed by the alloy. Moreover, the pure portion of the silver that entered the alloy had previously been fused in a similar crucible (one of the same nest), and with a similar mixture of pulverized charcoal and salt, and yet shewed no trace whatever of magnetic action. Hence, it could hardly be imagined that the magnetism displayed by the alloy was due to iron derived from the crucible.

34. After subjecting a portion of the alloy (32) to dilute nitric acid, and finding no iron in the solution, the surface of the metal was washed in clean water and thrown into dilute sulphuric acid, which, upon the principles of electro-chemistry, took up a portion of the copper; and, had iron been present, would have taken it up also. But no indication of that metal appeared by the test of gall-nut, nor by that of ferrocyanuret of potassium; but the formation of ferrocyanuret of copper was manifested in an eminent degree, though, previously to the introduction of the ferrocyanuret of potassium, scarcely any colour was perceptible in the liquor.*

35. From the facts above described (32, 33, 34),—and at present I rest on no other data,—I am inclined to think that

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* Another portion of the alloy has since been analysed, with similar results.

if any iron could possibly have entered the alloy, its quantity must have been too small to cause the high degree of magnetic action which the specimen exhibited.

36. On comparing this alloy of silver and copper with the alloy of iron and antimony, in which the weight of the latter metal is only about twenty times that of the iron (24), some very remarkable circumstances present themselves. In the former alloy, where no iron can be detected by the usual chemical tests, we have a metal whose magnetic action is, at least, twice as powerful as that displayed by the alloy of iron and antimony, an alloy of which iron constitutes a very considerable proportion, and whose presence, had it not been previously known, could have been detected by the humblest test for ferruginous matter. These parallel experiments tend to shew either that an alloy of pure silver and pure copper is magnetic, or that the magnet is a better test for the presence of minute portions of iron, in such alloys, than any hitherto known in chemical manipulations.

37. I regret that another piece of unmagnetic silver has not yet fallen in my way, to enable me to make further investigations on this curious subject. But I am in hopes of obtaining unequivocal results before the second part of this memoir is brought before the Society.

38. With respect to brass, one of our most important alloys, I have found it to be highly magnetic in a great number of cases, viz., in all the various states of newly cast brass, brass-wire, and sheet-brass, as well as in several articles of brass manufacture. But, from the very great difference in the degrees of magnetic action displayed by different specimens of this beautiful alloy, and the total absence of that action in others, there has appeared to me a high degree of probability that the magnetism displayed by brass is due to accidental portions of iron in the alloy.

39. Cavallo discovered that in magnetic brass the action was more powerful in large pieces than in small ones. This fact I have also observed in several specimens that I have examined. The reason seems to be, that in the larger pieces a

greater quantity of ferruginous matter (if present) is brought into operation than in the small pieces.

40. Cavallo also states, that hammering an unmagnetic piece of brass will cause it to become magnetic. An instance of this kind I have never yet met with ; but I have found that when an unhammered piece has been so slightly magnetic as to have that character but just discernible, hammering it so as to compress its two sides closer together, gives it an increased magnetic action, which may possibly be a consequence of bringing the whole of its magnetic particles more completely within the range of the testing magnetic influence ; and I am inclined to believe that, had Cavallo's test been more powerful than a magnetic needle, he would have found that those pieces whose magnetism he thought was due to compression alone were slightly magnetic previously. Still, however, there is a possibility that magnetism might be detected in compressed brass, in which that power is too feeble to be detected whilst the metal is in an uncompressed state, even by powerful magnetic tests.

41. In addition to the advice given by Cavallo, respecting the necessary caution in employing brass in the construction of compass boxes (note to 6), I should advise the makers of those useful instruments to test every piece of brass intended to be employed in their construction, by a powerful magnet, instead of testing them in the usual way by means of a delicate magnetic needle ; and if this test be accurately performed when the metal first arrives from the foundry, the detection of any concealed magnetism, even if very feeble, will be almost certain, and much of that labour and uncertainty, which must always attend examinations by the needle, would be avoided. Unfortunately, however, too much reliance is usually placed on the mere appearance of the brass, or on the character of the foundry whence it is procured, and the consequence is, that but very few brass compass boxes that are in common use are entirely free from magnetic action.

42. The next alloy of importance that I have examined is German silver, in which nickel is one of the principal consti-

tuents. In the best kind of German silver (constituted of copper eight parts, nickel six, and zinc three), a slight magnetic action has been detected, but in the inferior kinds of German silver, into which only about three parts of nickel enter, I have not detected any magnetic action whatever. Hence the magnetism of that portion of nickel is obviously neutralised in that particular alloy.

43. The metallic salts that I have examined, are some of those most frequent in common use. The salts of iron were the sulphate, the yellow and the red ferrocyanuret of potassium, also Prussian blue. These, with the exception of the yellow prussiate, are magnetic; the sulphate of iron in the highest degree of any of them. It is somewhat remarkable, that the two kinds of prussiate of potash, where the proportions of iron are so nearly alike (yellow 15 per cent., red 16 per cent.), should display such a material difference in their magnetic characters; and it is still more singular that Prussian blue, which contains more than 45 per cent. of iron, is less magnetic than the red prussiate of potash. The sulphate contains about 33 per cent. of iron, and is the most magnetic of the whole.

44. Pure sulphate of copper shews no magnetic action, but that of commerce is highly magnetic, being, as I have ascertained, adulterated with sulphate of iron. Hence, the magnet would be a good and speedy test for the quality of the commercial salt.

45. The following salts appear to be perfectly neutral to magnetic forces:—Common salt, saltpetre, borax, sulphate of magnesia, sulphate of soda, sulphate of potash, carbonate of potash, and carbonate of soda.

46. Thus far I have attempted to contribute to the list of facts previously known, and to correct some errors which, probably on account of the inefficient modes of investigation, have crept into this particular branch of science. It is possible also, I think, that some of the facts which have now been pointed out, may be an inducement to employ the magnet more extensively than hitherto in the laboratory of the chemist.

47. In addition to the facts enumerated in the first section of this memoir, further investigations have led to the discovery of others of a no less interesting character. By extending the examination of British silver coins, I find that the whole of them are more or less magnetic. The half-crowns of the present reign that have come under my observation are certainly all magnetic, and some of them display considerably high magnetic powers, more especially those of the years 1842, 1844, and 1845; and a Victoria shilling, coined in the year 1842, is still more magnetic than any of the half-crowns.

48. The half-crowns of George the Fourth are, in general, highly magnetic, though but very few of them display such high magnetic powers as some of those of William the Fourth. There is something remarkable in the following fact:—I have not met with any of the half-crowns of George the Third that are so powerfully magnetic as those of subsequent coinage.

49. With respect to silver ornaments, silver medallions, and silver articles for domestic purposes, they differ materially in their magnetic characters; but, generally, they are more highly magnetic than the British silver coins (30).

50. The large silver medal of the Society of Arts for the year 1825, weighs nearly two and a half ounces; yet it does not display even the slightest degree of magnetic action, by the most severe test to which I have subjected it; whilst a small silver medallion of the Commonwealth, representing Lord Essex on one side, and the two houses of parliament on the other, was found to be more highly magnetic than any of the previously named coins. A small silver medallion of Charles the Second, exhibited a slight degree of magnetic action. An Indian rupee that I examined, shewed no magnetic action whatever.*

51. The present gold coinage of this country is, in general,

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* For the use of these medallions and the Indian coin, I am indebted to my friend G. Waring Ormerod, Esq.

but feebly magnetic. I have met with a few sovereigns of the present reign which are more magnetic than any others that have come under my notice.

52. With respect to jewellery, it is generally more highly magnetic than those articles of silver that have come under my examination. Wedding rings, which contain but a small proportion of copper, have so slight a degree of magnetic action as almost to elude the detection of it; whilst ornamental rings, keepers, &c., which contain a much greater proportion of copper, are, generally, highly magnetic. Some ear-rings that I have examined, are still more magnetic than the finger-rings. Gold watch-chains are generally magnetic, especially those containing much copper: also gold spectacle frames, unless they be of what is called fine gold, are magnetic to a considerable extent. In all cases where steel or iron screws or nails have been found in the gold articles examined, those parts have been carefully removed previously to the magnetic test being applied.

53. We next come to the consideration of metallic alloys, of which either iron or nickel form no inconsiderable proportions. It has already been shewn in the first section of this memoir (24), that antimony, when alloyed with iron, counteracts the magnetic action of the latter metal in a very eminent degree, rendering it almost undetectable when the iron amounts to little less than a twentieth part of the mass: and I find that when the ferruginous metal amounts to no more than about one-fortieth of the mass, its magnetic powers entirely disappear.

54. There are several other metals, besides a number of other bodies, which either partially or wholly neutralize the magnetic actions of iron and nickel. The most eminent of the metals in this capacity is zinc. This metal, which till these researches were undertaken, was not known to affect the magnetism of iron, neutralizes nearly the whole of that power when alloyed with an equal proportion of the ferruginous metal. And, although from an accident with the melting-pot, I have not yet arrived at the fact, I have no doubt whatever that, when the iron amounts to no more than one quarter of

the mass, its alloy with zinc will be perfectly neutral to the magnet.*

55. The neutralization of magnetism in iron by alloying it with zinc is a fact of high importance in the contemplation of metallic magnetism; and especially the magnetism of brass and other alloys, in which zinc forms a considerable proportion. For it is highly probable that, since zinc smothers the magnetic influence of large proportions of iron, a considerable quantity of the latter metal might enter the composition of brass, without rendering it palpably magnetic. Such, in fact, would absolutely be the case, provided the alloy were perfect, and that the copper had no influence on the magnetic condition of the combined iron and zinc.

56. To satisfy myself on this point, I have subjected to chemical analysis some of those specimens of brass which had been found to be highly magnetic; and, as far as I have proceeded, there appears no reason to suppose that the magnetic powers they displayed were due to iron in their composition. Indeed, I am now inclined to embrace the opinion of Cavallo (6, note), that the magnetism of brass is not due to ferruginous matter, but depends upon a suitable arrangement of the particles of its proper constituent metals, copper and zinc. Nor do I believe that brass generally, as it leaves the foundry, contains any notable quantity of iron. I have analysed many specimens, both magnetic and unmagnetic, and the traces of iron, where any were discoverable, were very minute, and as frequent in the one kind as in the other. It is true that some specimens of brass contain more than an average proportion of iron; but it is a curious fact that these specimens are not those which display the greatest magnetic powers.

57. We learn, also, from these facts, that the demagnetizing powers of antimony and zinc will necessarily prevent the detection of small proportions of iron in those metals,

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* Since this paper was read, I have ascertained that an alloy of iron and zinc in the proportion of 1 to 7 respectively, is quite destitute of magnetic action.

even by the aid of powerful magnetic forces, and leave us in uncertainty regarding their purity when examined by this test alone.

58. Lead and iron do not easily unite into a perfect alloy, excepting when the ferruginous metal is in very small proportions; but when thus combined, the iron loses a great part of its natural magnetic qualities.

59. Silver and copper unite very sparingly with iron; but whether the magnetic powers of the latter are affected by its union with those metals or not, is not yet known.

60. In order to ascertain the exact quantity of pure metallic iron that would render a neutral half-crown[*] *apparently* magnetic, to the same extent as another half-crown was *absolutely* magnetic, I attached to the former, by means of softened gum, new iron filings, and after many trials ascertained that the requisite quantity of filings amounted to about a ten-thousandth part of the mass; and, on changing the pure iron for the peroxide of iron, a 480th part of the mass was required to render it equally magnetic with the standard half-crown. Now, as more than two-thirds of this oxide is iron, it follows that, in this state, the iron loses a considerable portion of its magnetic powers; and that the proportion of iron required in this case to produce the standard degree of magnetism to the mass, was little short of a 700th part.

61. Now, if any iron existed in the standard magnetic half-crown, uncombined with the other metals, it must have been in a state of peroxide,[†] and that more than an 800th part of the half-crown must have consisted of iron, if its magnetism were due to the presence of that metal.

62. Again, the magnetic action of this half-crown was considerably more feeble than that of the alloy which has been chemically examined; and in which, if iron were present at

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[*] This piece of coin was not entirely devoid of magnetic action, but it approached nearer to a state of neutrality than any other I then had. Its action was very feeble indeed.

[†] It is possible that the iron, if any, might be in the state of a carburet; but, even in this condition, much of its magnetic powers would be neutralized.

all, that metal was in a less proportion than a 20,000th part of the mass, which proportion, in a state of peroxide, and divided, as it necessarily must have been, through the whole alloy, would scarcely yield the slightest perceptible magnetic action. Moreover, if iron to that amount were even pure or uncombined, its quantity was far too small for the display of those high magnetic powers of which it was obviously possessed. And as there is a probability at least, that the magnetic powers of iron become deteriorated by an alloy of that metal with silver or with copper or both, there is not the slightest reason for supposing that the magnetism of the alloy in question (32) was due to any iron that it could possibly contain. Nor do I believe that the magnetic actions displayed by the coinage are traceable to the presence of iron.

63. It has already been stated (42) that the magnetic action of nickel is considerably neutralized when combined with zinc and copper in the alloy constituting German silver. Since that part of this memoir was read before this Society, I have had an opportunity of alloying nickel with zinc alone, and have ascertained that when the zinc is about eight or ten times the quantity of nickel, the alloy is perfectly neutral to the magnet. This alloy has a zinc-coloured fracture, and partially crystallized in the manner of zinc; but it is extremely brittle and easily pulverized in a mortar.

64. Nickel and antimony combine with facility, and in an extraordinary manner. If two pieces of the metals, one of each, be placed side by side in the crucible, so as to touch one another, especially at their upper ends, the moment the antimony assumes a dull red heat, even a lower heat than that which commences its fusion when alone, the nickel bursts out into a fine scarlet glow, fuses and spreads over the antimony in a beautiful fluid state, and insinuates itself into the pores of that metal, rendering the whole mass soft like paste or butter. If, whilst in this state, the crucible be removed from the fire and permitted to cool gradually, the fracture of the button of this alloy, when broken, is of a much lighter colour than that of antimony. It is of a light grey, and very imperfectly crystallized. It is not so brittle as

antimony, though still pulverable in a mortar. When one-fourth of the mass is nickel, the fracture is very compact, and not unlike that of fine steel, but of a lighter colour. With these proportions the alloy is somewhat malleable, and can be cut by a cold chisel.

65. From a retrospection of the facts developed by these researches, in connection with those previously known, we are led to observe a material difference in the magnetic characters of bodies when in their simplest or natural conditions; and that these natural magnetic characters become considerably modified when the simple or elementary bodies are variously combined; some simple bodies losing their natural magnetic properties, and others displaying a new magnetic action of which, before combination, they appeared to be destitute. Under these circumstances, it would be difficult to ascertain the line of demarcation between those bodies that are naturally and separately magnetic, and those that are not. Probably the safest way would be to allow all bodies to possess more or less of the magnetic character, and to classify them into those that are *palpably* magnetic, like iron and nickel, and those that are but obscurely magnetic, or whose magnetism is not detectable in their individual states, but which become magnetic by combination.

66. Provisionally, therefore, we might venture to call the former class *Sapho*[*] *magnetics*, and the latter class *Asapho*[†] *magnetics*.

67. *Sapho-magnetics* might be conveniently subdivided into *Mono*[‡] *magnetics* and *Suno*[§] *magnetics*, according as they consist of individual or of compound bodies. Then, as we have many bodies which counteract the highest magnetic powers of simple bodies, these might be called *Kato-magnetics*,[‖] because many of them, if not all, have the power

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[*] Σαφα. Clearly, manifestly.

[†] Ασαφως. Indistinctly, without clear evidence or marks.

[‡] Μονος. Alone, single.

[§] Συν. Together, or, Συναιρω. To co-operate.

[‖] Κατα. Opposite to, to make disappear.

of completely neutralizing the magnetic actions of other bodies.

68. The *Mono-magnetics* at present known are but few in number, iron being the grand type. Next to iron is nickel. Cobalt is also a mono-magnetic body, and, at present, completes the list of this class of magnetics.

69. In the *Suno-magnetic* class I place alloys of copper and silver, copper and gold, and copper and zinc; and although these three are the only ones with which we are acquainted, I have no doubt that many more alloys will soon find a place among suno-magnetics.

70. The *Kato-magnetics* are very numerous, as this class includes all bodies which, by combination, impair the magnetism of other bodies. Amongst the metallic *Kato-magnetics,* zinc is the most powerful hitherto ascertained. Next to zinc is antimony. Then lead and tin. Arsenic, probably, stands very high in this class, but I have had no opportunity of ascertaining its proper place. The non-metallic *Kato-magnetics* are sulphur, oxygen, cyanogen, chlorine, carbon, and the generality of those bodies which combine with the metals.

71. In proposing this classification of magnetics, I have aimed at nothing further than an abstract of that which absolutely takes place in nature. The whole rests upon facts, most of which have their analogies in electricity. All bodies are known to possess electric properties, but differing in degrees of power, and the compounds display very different electric powers to those of the simple constituents. The electro-magnetic powers differ in different bodies, both simple and compound, as decidedly as the powers which are purely electric. Therefore, this classification may be considered as supplying a small portion of an extensive nomenclature that has long been wanting in this region of science.

Twelfth Letter on Glaciers. Addressed to Professor Jameson.

By Professor J. D. FORBES.[*]

On the Extraordinary Increase of the Glacier of La Brenva from 1842 *to* 1846*—its Motion—Experimentum crucis respecting the Origin of the "Veined Structure."*

MY DEAR SIR,—In continuation of the results of my recent journey, of which I communicated a part in my Eleventh Letter, I shall first give some account of the phenomena observed on a fresh visit to the glacier of La Brenva, on the south side of Mont Blanc. Having undertaken the journey from Chamouni to Courmayeur and back, for the sole purpose of examining anew this glacier and that of Miage (of which I have already given a full account in the tenth chapter of my *Travels in the Alps of Savoy*, &c.), I thought myself fortunate in obtaining some important and unexpected observations, as well as in again meeting M. Carrel, canon of Aoste, who, in company with M. L'Eglise, canon of the Great St Bernard, very kindly made a visit to Courmayeur, on purpose to join me, and afterwards accompanied me upon the glaciers.

Upon arriving opposite to the glacier of La Brenva, on the 6th August last, in descending from the Col de la Seigne, I was first struck by the surprising extension of volume which it had undergone since my last visit in 1842. This will be understood by a comparison of the limit of the glacier in 1842, as sketched in the plan No. II., opposite to page 193 of the first edition of my Travels, and reproduced in Plate II., fig. 1, accompanying this paper, and the line of its extension in 1846, marked by a dotted line in the same figure. I wish it to be understood that the sketches in question being only taken by the eye, have no pretension to exact accuracy, but fortunately the land-marks are sufficiently distinct to prevent the least dubiety. Thus, for example, the bay or hollow opposite to C in the sketch, and which was drawn, in 1842, as filled with the old moraine of 1818, was filled up with ice very nearly, if not quite, to the level of that moraine, so as to follow the natural curve of the soil, which it everywhere touched, presenting a steep wall of nearly unbroken ice, 70 feet in height, facing the bank. Again, opposite to the remarkable chapel of *Nôtre Dame de la Guérison,* of which I have given an account in my Travels, with the history of its invasion and ruin by the progress of the ice in 1818, the ice has risen against the projecting rock, beneath the old larch tree (seen in Plate IV. of my Travels, and in Plate I., fig. 1, accompanying this paper), and seems to threaten the security of the path, in the same manner as it did at that time. "The height of this rock," I stated in 1842, "is now about 300 feet;" in August 1846 it was scarcely more than 100! and as the glacier towered up to a great height beyond, it had all the appearance of menacing the pathway, and once more tearing up the limestone rock. But this will be still more distinctly conceived by referring to Plate I., figures 1 and 2, which shew the size of the glacier in 1846 contrasted with the view of 1842. Both these sketches were taken from the same spot (marked A on the ground plan), and as I had not, on the latter occasion, my former drawing for comparison, the evidence of the increase is the more striking, and the accuracy of outline of the fixed parts of the landscape is confirmed. The increase of height and length of the glacier, as well as its breadth, is here well marked. In consequence of this great accumulation of ice (accompanied, probably, by an increased velocity of motion), the surface of the glacier is exceedingly crevassed, in some places even divided into pinnacles, and, generally, incapable of being traversed without great difficulty, although, in 1842, I could have walked over it in almost every direction.

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[*] Read to the Royal Society of Edinburgh, 7th December 1846.

The cause of this very surprising increase is a general one, since most of the glaciers which have been recently examined, present it in a more or less striking degree; for instance, the Glaciers des Bois and Bossons, in the valley of Chamouni.* The cause is no doubt to be sought partly in the great fall of snow of the two winters 1843-4 and 1844-5, and the cold wet summers which followed them. The immediate effect of the snow is to protect the ice and diminish the annual ablation. Hence the glacier shoots farther into the valley before the waste suffices to equalize the supply. In the cold spring and summer of 1845, this effect appears to have been most conspicuous, as appears from the decisive observations of Balmat, which I have elsewhere published.† The swollen condition of the Mer de Glace of Chamouni in 1846, was shewn by the fact, that, in spite of the intense and continued heat, it was much higher opposite to the Angle in the middle of August, than it was in June 1842, most of the marks which I then made, for the purpose of estimating the progress of the glacier, being covered by the moraine at the latter period. The extremity of all the glaciers of which I have obtained information, was advancing towards the valley during the summer of 1846; and this was even accelerated by the great heat of the season. For though it must increase the ablation of the surface, and the melting of the terminal face, and thus diminish the mass of ice, its immediate effect is to fuse the glacier into a state of pliancy, such as to increase its motion in a very perceptible manner (as I have established by direct experiment), and thus discharging its icy burden into the valley faster than even the increased atmospheric heat is capable of dissolving it, it spreads with a velocity which, if it could be supposed continual, could not fail to be alarming. Thus it appears, from certain observations made at the desire of M. Carrel, by M. Guicharda, vicar at Courmayeur, that the *snout* or extremity of the glacier of La Brenva, has protruded into the valley no less than 22 mètres, or about 60 feet during the two months of summer, being at the rate of *a foot a day*. The result of this advance is, that the glacier is rapidly attaining the old moraine of

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* A very striking evidence of this change has occurred at Chamouni. The torrent Arveiron, instead of issuing from beneath the bed of the Glacier des

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Bois at its termination, escapes chiefly at a much higher level, and formed, in 1846, a striking cascade on the west side of the glacier, nearly opposite to the Chapeau. A similar occurrence is said to have happened about 30 years ago.

† Phil. Trans., 1846, p. 183.

1818 in the bottom of the valley, from which it is now only about 100 yards distant (see the Plan, Plate II., fig. 1), whilst it is tearing or ploughing up the soil on the southern bank, marked in Plate I., fig. 2, on the left hand side.

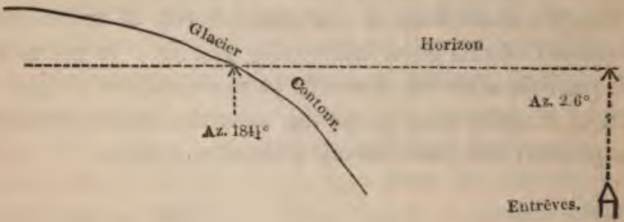
The same gentleman, M. Guicharda, has himself made, with considerable labour, observations intended to test the reality of the movement of the glacier during winter, which confirm, in every particular, those which I have already published regarding the glaciers of Chamouni. The movement appears to be very regular; but, from the position selected for the measurement (the terminal face of the glacier), where the friction is most intense, and the movement slow (see Eleventh Letter), and where the danger from falling stones is such as absolutely to prevent the continuance of observations during summer, they are less comparable and complete than would otherwise have been the case. Having explained fully to MM. Carrel and Guicharda my methods of observing, and pointed out a more convenient site, we may hope that we shall have a continuous series of trustworthy observations of this most interesting glacier. In the meantime I publish, with M. Carrel's permission, the degree of motion of a stake fixed in the ice, near the lowest extremity of the glacier, and little elevated above the soil.

1845.	h.			h.	Motion.		Daily Metres.
Nov. 25,	9½	to	Nov. 28,	1½	0·42	metres.	0·13
... 28,	1½	to	Dec. 1,	1¼	0·38	...	0·13
Dec. 1,	1¼	to	Dec. 11,	12¾	1·127	...	0·113
			1846.				
... 11,	12¾	to	Jan. 7,	2	3·23	...	0·12
1846.							
Jan. 7,	2	to	. 30,	11¾	1·735	...	0·075
... 30,	12	to	Feb. 9,	12	1·58	...	0·15
	*		*			*	
Mar. 21,	1	to	Apr. 2,	10	1·18	...	0·10

The mean daily motion is about 5 English inches, and it is probable, from the discrepancy of the 5th and 6th observations, that the measurement of the 30th January was faulty.

If this be so, the general uniformity, as well as smallness of the motion, is accounted for by the important retardation due to friction.*

In the absence of an exact geometrical plan of the glacier, it was important to preserve some accurate record of its extension at the time of my visit in August 1846, which was done in the following manner:—The theodolite stationed at the point B, Plate II., figs. 1 and 4, that is, on a promontory of limestone a little to the west of the chapel of Nôtre Dame, and close to the path from Courmayeur to the Allée Blanche. It is also 57 feet to the eastward of the old larch seen in the views, Plate I. From this point, then (which cannot readily be mistaken), the telescope of the theodolite was directed upon the steeple of the little church of Entrêves, the azimuth marked (286°), and the telescope was exactly levelled. It was then turned in azimuth (tracing out the horizontal plane), until it cut the contour of the glacier of La Brenva, which was at an azimuth of 184¼°. The difference of azimuth 101¾°. It is plain that if the glacier advances bodily into the valley this angle will diminish; if it retreats, the angle will increase. Farther, from the same station, B, the elevation of the highest part of the front of the glacier, where it crosses the valley, was 11° 37′.



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* It is to be noted, that the advance of the glacier of 60 feet in two months, mentioned in page 96, is not the motion of *a point* in the ice, but the protrusion of the front of the glacier after the effect due to melting has been deducted.

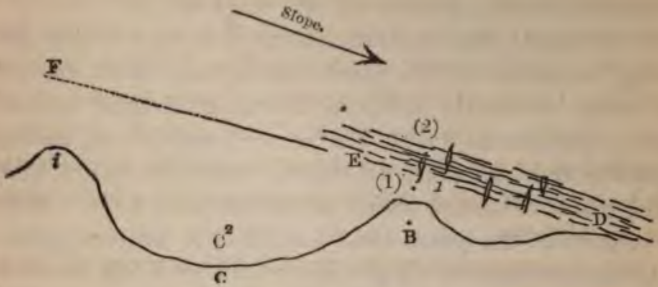
To fix the position of the lowest point of the glacier in the valley, a line joining the vault of ice from which the river Doire issues, and the door of the chapel of Nôtre Dame (observed from below), runs S. 42° W. magnetic.

On the whole, the observations of this year on the surprising extension of this glacier during the short space of four years under the influence of meteorological circumstances, peculiar no doubt, but scarcely anomalous, confirm amply the remarks which I formerly made (Travels, p. 205), on the great extension which it underwent in 1818. Were the climate of the years 1844 and 1845 to become permanent, the increase of all glaciers would evidently be enormous. In fact, a more turbid and cloudy atmosphere, with an increase of the usual precipitations, would suffice to increase glaciers to almost any extent; a great degree of *dry cold* would not produce the desired effect (viz., an extension which would account for the erratic phenomenon), a temperate climate being most favourable to the growth, especially the progression, of glaciers.

Veined Structure. Glacier of La Brenva.

I must now proceed to notice an observation of a very interesting kind which my visit of 1846 to this glacier enabled me to make, and which seems perfectly conclusive as to the truth of the explanation which I have elsewhere given of the origin of the veined structure, so important to the correct theory of glacier motion. The present appears to me to be an *experimentum crucis;* I can only hope that it may remain in existence long enough to convince all those who have any remaining doubts on the subject.

The glacier of La Brenva is distinguished by the beauty of its structure. I have described it in my Travels, p. 202, and endeavoured to represent it in Plate V. of that work. This was as it existed in 1842. But now that the ice has risen up against the promontory B (Plate II., fig. 1), and has filled up the bay C, the structure opposite to B has become developed in the most remarkable and beautiful manner. The plates of green and white ice alternating in the



direction of the lines E D, not close to the foot of the rock at B, but at a little distance, the veins E D running in the direction of the declivity of the glacier, and *beginning* to be developed at E, becoming more and more so towards D, where they form a tangent to a swelling surface of rock, whose resistance evidently gives rise to them. It seems clear that all the ice within the line F E D, or between it and the shore *i* C B D is *embayed*, as it were, and has but little motion, in consequence of the intense pressure with which the whole mass of the glacier is urged against the side of the valley; but as the glacier is finally compelled to move in the direction of the declivity from E towards D, a longitudinal tearing force arises parallel to E D, and the motion is facilitated by the formation of the veined structure already mentioned, for of longitudinal crevasses there are *absolutely none.* Such crevasses as there are, are *transverse to the blue bands,* shewing the usual direction of the tearing force due to the lateral friction. But they are small and isolated.

Now, in this case, the development of the structure is evidently due to the projection at D in contact with which it terminates, and though the whole glacier is more or less structured in the manner described and figured in my former work, this remarkable development gradually ceases about E, where the rush of the ice-current past the promontory B has ceased to exert so palpable an effect. I can positively state that no such unusual development of the structure occurred opposite to B in 1842, and, for this reason, that the glacier lying then more in the trough of the valley, not being violently pressed against the promontory B and embayed in the height C, the friction and longitudinal tearing force was incomparably less. I wish I could convey any adequate idea of the beauty of the ice for an extent of some hundred feet in length, and for a comparatively trifling breadth between D and E. It resembled the greenish veined marble called by the Italians *Cipollino*, when of the highest perfection, and polished or wetted; and it was impossible to resist the wish to carry off slabs, and to perpetuate it by hand specimens in cabinets.

I did not, however, content myself with arguing the relative motions of the parts of the ice and embaying of the rest, from the mere configuration of the ground. But I made the following experiments, which proved that my first belief was correct.

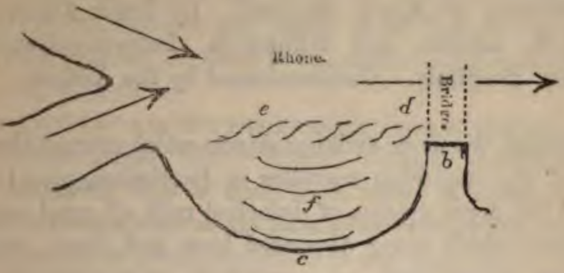
The instrument being stationed at B, and a transverse visual line established with reference to an object on the farther side of the glacier, two vertical holes were made at (1) and (2), the first on the nearer, the second on the farther side of the remarkable veined structure already described. If that structure was occasioned, as I suspected, by the rapidity with which the farther portion of the ice was moved past the nearer portion, such difference of velocity might be expected to be observed in a marked manner. The mark (1) was about 50 feet from the nearest edge of the glacier, and 103 feet below the station B. The mark (2) was about 170 feet farther and 60 feet higher. An approximate section of this part of the glacier, together with the measurements on which it is founded, is given in Plate II., fig. 4, and will serve to compare the state of matters at any future time. The following Table gives the result of the motions for two days:—

		Motion.			Motion for 24 hours.	
		No. (1.)	No. (2.)		No. (1.)	No. (2.)
1846.	Hour.	inch.	inch.		inch.	inch.
August 8,	9½ A.M.	5·7	15·75		5·26	14·54
... 9,	11½	3·7	11·25		4·55	13·85
... 10,	7					
	Sums,	9·4	27·0	Means,	4·90	14·2

consequently the velocity of motion had increased in the space of 170 feet traversed by the veined structure, in the ratio of no less than 29 to 10.

I also examined the condition of motion of the embayed ice in the position C on the ground plan, of which a section (on a scale much larger than the last) is given in fig 3. It is made through the visual line, or in the direction C, C 2 of the plan. Two pins were fixed at C 1 by excavating a niche in the nearly vertical face of ice; one was placed vertically, the other horizontally. At C 2, 40 feet higher and 69 feet more distant, a vertical pin was placed. The diagonal from C to C 1 was also accurately measured with a line. After more than 24 hours interval it was found that the two marks at C 1 had not moved towards the right hand, or in the direction of motion of the glacier by the *smallest perceptible quantity;* and that mark C 2 had advanced by only a small fraction of an inch (0·2); but on the other hand the vertical pin at C 1 had approached the station C *by two inches;* shewing that the motion is here entirely directed *outwards and upwards* against the bank at C, *exactly as an hydrostatic pressure acting on a plastic mass would occasion.* The result was the more interesting because it was altogether unexpected. It had not occurred to me that the embaying of the ice could be so complete as to leave no appreciable effect due to the drag of the central ice in the direction of the declivity.

The precise analogy of the phenomena now described to what obtain when the motion of a stream of water is interrupted by lateral obstacles, will suggest itself to almost every one; but to take an illustration which is not imaginary, I have given, in the annexed figure, a sketch of what occurs in the course of the river Rhone at the bridge near Sierre.



The course of the current is indicated by the arrows, the pier of the bridge *b* embays the water at *c*, where it is whirled about by the tangential action of the current but does not escape, the wavy lines *e d* indicate the ripple produced by the friction of the central past the lateral portions of the stream, the disturbance from which is propagated towards the shore in the waves *f*. All that is here described may be still better seen in a sluggish stream, and if it be covered with any kind of scum, it will be torn up into longitudinal shreds between *e* and *d* exactly corresponding to the position of the icy bands. The nature of the force producing these lines of scum is also well shewn by the rotatory motion acquired by any floating patches of foam, which is always in a direction forwards and towards the side of the stream to which it is nearest; thus, in the preceding figure, it would be in the direction of the hands of a watch. I remain, my dear Sir, yours very truly,

JAMES D. FORBES.

EDINBURGH, 24*th October* 1846.

Erratum.—In the Eleventh Letter, in the Ed. Phil. Journal for October, p. 421, line 18,

for $\sqrt{9 + (9 \text{ cotan } \theta - p \text{ cosec } \theta)^2}$

read $\sqrt{9^2 + (9 \text{ cotan } \theta - p \text{ cosec } \theta)^2}$

Note on certain detached Rocks in Barbadoes, and on the Formation of Agaric Mineral. By JOHN DAVY, M.D., F.R.S., London and Edinburgh, Inspector-General of Army Hospitals. Communicated by the Author.

Amongst the more striking objects which attract the attention of the inquiring traveller in Barbadoes, may be mentioned the great masses of detached rock of coral and shell limestone which occur dispersed here and there, chiefly in its north-eastern part,—a part which, as regards its strata, may be considered analogous either to the old red sandstone, or, at latest, to the new red sandstone formation; and which, as I have already pointed out in a former communication, is remarkably contrasted with the other and greater portion of the island, and I may now add, with those masses of rock that may be held to represent this portion. Between the two parts there is a limitary line of inland cliff, similar in its structure to the detached rocks. It is generally supposed that these masses are derived from the cliff, broken off, and somehow removed to the spots where they are now found. That this is their history in many instances can hardly be doubted; as where they are met with under the cliff, or even at the distance of two or three miles, where an inclined plane intervenes. But there are other sites on which they are to be seen, to which such an explanation is hardly applicable, —as on rising grounds and hills, with hollows and valleys intervening, over which, by the mere force of gravity in falling, they never could have passed,—leading to one of two conclusions, either that the valleys referred to have been formed since the detached masses were separated, or that other causes have been in action besides those presumed in the foregoing supposition, but of what kind, whether glacier or volcanic, it may be difficult to determine.

These detached rocks are commonly covered with wild plants, and, in consequence, with their steep sides and often castellated forms, have a very picturesque appearance in the landscape, especially contrasted, as is often the case, with the uniform surfaces of rich sugar-cane cultivation by which they are surrounded. In two or three instances they are found on the sea-shore; and there, no doubt owing to the influence of the copious salt spray from the waves which break against them, or near them, they are as remarkable for their nakedness.

On the sea-shore in the parish of St Joseph, near the outlet of a rivulet called "Joe's River," is one of these masses. It may be about fifty or sixty feet high. I paced it, and found its circumference at its base to be seventy-three of my ordinary paces, or about one hundred and nine feet. When the tide, which rises here from two to three feet in perpendicular height, is high, it is surrounded by the sea; and it bears on its base seaward, diminishing from thence, landward, the impress of the action of the waves, in the manner in which it is worn away. On the land-side the effect is most trifling; but on the sea-side it is so considerable, as to form there an overhanging roof, gradually heightening to a certain line from the base, under which I could stand erect, and so regularly worn, that were the rate of corrosion for a certain period ascertained, the length of time it had been so acted on by the waves might be determined probably with some precision.

A single shrub grows on it, on its landward side, where most sheltered from the sea spray. On its south-eastern side, where, in some shallow cavities and fissures there is an oozing of water (derived, no doubt, from rain falling on the surface above), a mucor is there found, in part green, in part blackish, where least exposed to the direct rays of the sun, mixed whith a wite matter having the character of the agaric mineral of mineralogists.

This last mentioned substance is of a pure opaque white; when dry it is light, and easily broken and crumbly. Under the microscope (prepared by being crushed on glass mixed with a little water) its powder exhibits aggregate globular masses, and particles nearly globular in form, varying from about $\frac{1}{2000}$ of an inch in diameter to $\frac{1}{500}$; somewhat resembling, in appearance, certain compound organic globules, frequently met with in the lungs after inflammation, and in other inflamed parts; and thus presenting, as it were, a link in this resemblance between inorganic and organic nature. Chemically examined, it was found to consist chiefly of carbonate of lime, with a little carbonate of magnesia, and a trace of silica and vegetable matter.

The likeness of this substance to agaric mineral has led me to conjecture that the agaric mineral itself may owe its origin to the same causes or circumstances as this substance, which I believe to be the deposition of carbonate of lime on a low form of vegetation, such as mucor, itself prone to decomposition, and probably yielding a portion of carbonate of lime, and it may be the whole of the carbonate of magnesia, and the silica in the process of decomposition. A portion of the green mucor that I detached, and brought away, I found, on exposure to the sun, to become white, which may account for the perfect whiteness of the incrustation, though containing a minute portion of vegetable matter. The presence of this vegetable matter may be a modifying circumstance in relation to the light and crumbly state of the deposition, and its peculiar structure. I found that when it was acted on by a dilute acid, the solution formed contained a little of this matter, indicated by a greenish hue being imparted on the addition of ammonia; and, farther, that when the lime was thrown down in the form of carbonate by the addition of sesqui-carbonate of ammonia, it did not adhere to the side of the glass vessel, or to a glass rod, as it usually does, and on examination by the microscope it was found to be in globular particles varying in diameter from $\frac{1}{2000}$ to $\frac{1}{4000}$ of an inch.

In a former paper* I have noticed the fact, that the presence of some vegetable matter in solution has prevented the separation, in the form of crystals, of a very crystallizable salt,—the double phosphate of magnesia and ammonia, and have referred to it in explanation of the amorphous state in which urinary calculi often occur. The same fact may apply to carbonate of lime in other instances than the one above mentioned; and if we suppose that the substance of chalk has been deposited from solutions, and that the fluid from which it has been precipitated contained some matter that had been organized dissolved in it, it may help to account for the peculiar form of chalk, which, in its structure, is an approach to agaric mineral. This idea I venture to propose as a conjecture on an obscure subject. I have endeavoured to test it by precipitating lime, as a carbonate, from solutions with which an infusion of some vegetable or animal substance had been mixed; but the results have been ambiguous,—some apparently in accordance with the notion, some contrary to it. Some facts, which are well known, may be adduced as seeming to favour it. The cleanest sand, most free from impurities, is required to make the strongest mortar. The carbonate of lime of sea-water is not found to be deposited on marine vegetables, nor on shores where there are impurities brought down from the land by streams—a happy circumstance, allowing of their dispersion—but on coasts, and in situations where the water is clearest, and the sand cleanest, and where the waves break with the greatest violence,—all circumstances, I believe, favourable, both to the deposition of carbonate of lime, by the separation of its solvent carbonic acid, and to its crystallization, and thereby to its cementing quality. In the paper already referred to, in which some cements of carbonate of lime, of unusual purity, found in ancient buildings, are mentioned, of chalk-like softness and texture, the opinion is hazarded, that that chalk-like condition may have been owing to the purity of the material; and considering that calcareous mortars, without sand, consist, in the first instance, of hydrate of lime in its solid state, its particles being granular (so they appear under the microscope), may favour an amorphous rather than a crystalline formation of carbonate of lime; whilst the reverse may be the case, and would seem to be so, when carbonate of lime is deposited, pure and unmixed, from a solution. And the traces of organized matter which are found commonly in chalk, either distinctly in the remains of the silicious skeletons of infusoria, or, indistinctly, in its acquiring a greyish hue when heated to a certain degree, which it loses, if the heat, with exposure to the air, be longer continued, may be mentioned, in addition, in favour of the conjecture of the modifying power of such matter (of low organization ?) on carbonate of lime, tending to prevent its crystallization, and to impart to it the chalk-like character.

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* Miscellaneous Chemical Observations, in Philosophical Journal, vol. xli., p. 263.

BARBADOES, *November* 6, 1846.

Observations on the Extent and Rate of Changes of Temperature of the Waters in the Estuaries of the Mersey and Clyde. By Mr RITCHIE ADIE of Liverpool. Communicated by the Author.

The observations here detailed are a continuation of those given in vol. xl., p. 307 of this Journal, with the stations, the means employed in taking the temperatures, and the time, exactly the same as before described. The present addition will now extend them through winter and summer seasons of a more opposite character than is usually to be met with in a much longer period of time. For the winter of 1844-5 was a protracted and severe one; the summer of 1845 was generally cloudy and cold; then the winter following, 1845-6, was mild and open, while the summer just gone was one of the warmest experienced for many years; and, indeed, the cold of the early part of March 1845, and the heat succeeding it in fifteen months after, namely, midsummer 1846, may be taken as the two extremes of our climate.

In February 1846, the temperature of the Mersey water ranged from		42°·9 to 45°·2
March	... at the beginning of the month,	47·1
	towards the end,	43·9
April	...	43·7 to 49·
May	...	50·3 to 56·6
June	...	60· to 69·
July	...	62·5 to 63·2
August	...	67·5 to 63·2
September	...	64.4 to 59·7
October	...	59· to 49·6

The highest temperature of the water of the Mersey was observed on 21st June, when it reached 69°; for some days it slowly approached this temperature, while the air in the shade was varying, at the hour of its maximum, from 78° to 80°, with little or no wind. On the 24th June, with a moderate WNW. wind, the water had fallen to 66·6, shewing a rate of change of 0·8 per day from the maximum summer heat. When the spring-winter of 1845 broke up, towards the end of March, the rate of change was 1° per day, which must be looked on as quite the extreme; for through the sultry weather of the past summer, I have had good opportunities of observing the rates of change of temperature, which, for several days taken together, often came near 0·8 per day, but never exceeded that quantity. In the green-coloured and more transparent water of the Irish sea, outside the estuary, the temperature was 63° at the time that the tidal waters which pass and repass before the dock-wall of the port were 69°. In the fresh-water ponds in fields around Liverpool, a similar difference existed between those shaded by foliage and those open to the sky,—the shaded ones corresponding in temperature to the waters outside of the estuary, the open ponds to the water among the banks.

		°
In the Clyde, nearly opposite the entrance to Loch Long, on	30th June, the temperature was	54·5
...	6th July	62·
...	19th Sept.	56·4
Largs,	13th	61·5
...	19th	58·5
Brodick Bay,	15th	61·3
...	18th	60·
...	19th	61·

The circumstances attending the rapid change noted between the 30th June and 6th July, deserve to be noticed, for the heat of the weather had no part in it. From the end of June till the 5th of July the sky was mostly overcast, with heavy showers at intervals, and the temperature so much below what it had been in June, that there was no reason for supposing that the deep waters of this part of the estuary could be much changed in temperature by radiation. The rise of 7½° was due to a thunder-storm which extended over

Clydesdale on the afternoon of the 5th, and morning of the 6th; during the short space of fifteen hours, rain was deposited on the soil to a depth of 2 inches; in consequence of which, every stream and rivulet was swollen to the fullest extent. These speedily changed the estuary water from its usual transparent appearance, into an opaque, dull-red, muddy hue, and produced the increase of temperature over a large surface,—a result, arising from a storm, which it is probable that a week or a fortnight of the most brilliant summer sun could not have effected; for the observation on 30th June only exceeded by half a degree that of the previous summer, while the Mersey water was considerably warmer than the hottest period of 1845.

On comparing the greatest temperatures of the two last summers it will be seen, that this year, although the rates range higher from June to September, by the end of October the influence of the warmer season had lost its effect on the temperature of the Mersey water, which was then down to a point a little below the coldest of October 1845.

In my former communication I alluded to the appearance of luminous crested waves being connected with the chemical action of the water absorbing oxygen at the moment of breaking. In Brodick Bay, in Arran, during the calm, hazy weather of the third week of September, I noticed an appearance on the surface of the water which tends to strengthen this view, although I would by no means wish to state it as the sole source of luminous waves, when it is so well ascertained that many of the marine animalculæ can emit phosphorescent light.

In Brodick, the weather had been very calm for some days, the temperature of the water on the 15th, 16th, and 17th, was 61°. During the 16th, the sea had a glassy surface, and so transparent, that objects at the depth of 10 or 12 feet were seen distinctly. In the morning and evening, I observed on the shore, at two places above a mile apart, that where any object broke the smooth surface of the sea, there appeared to be a mixing together of two transparent media of different densities; where a projecting piece of rock, by the tidal current, produced a slight motion among the particles, it was seen distinctly; it was also very apparent when the surface of the water was struck with the hand. When the hot air from a chimney mixes with the colder atmosphere, or when two transparent fluids of different densities are mingled together, the same change is seen; and there could be no doubt whatever, that at the time in question, there were two films of water, of different densities, which were mixed when the surface was broken. What occasioned these two strata of water I am not prepared to speak of; but should further observations in other localities where the water is clear and occasionally very smooth, shew that this is a general, and not a rare or partial state of the surface of sea water, it appears to me to be extremely probable, that luminous crested waves by night accompany like conditions.

About the time I was engaged with these observations, I met with an amusing instance of the belief, among sea-faring men, of the power of the moon's rays to dispel thin fogs. I had remarked to the look-out man on the gangway of a steamboat, that I supposed the thin fog then resting on the surface of the sea had been troubling them for the last eight or ten days? He answered it had. Then, I suppose by night it troubles you the most? "Yes, Sir, but the moon has all the time been shining very strong, and she scratches it up pretty well." This belief in the influence of the lunar rays over thin vapours, is quite in accordance with what I have often heard of from sailors; and now that M. Melloni has clearly established the fact of a calorific power in the rays from the moon, it becomes interesting to inquire, whether or not this popular observation has correctly anticipated the careful researches of men of science.

The Industrial Arts of the Esquimaux. By RICHARD KING, M.D.[*] Communicated by the Ethnological Society.

Of the industrial arts of the Esquimaux, their habitations, in construction and variety of material, display, perhaps, the greatest ingenuity. Their hunting grounds extend about sixty miles inland, but their dwellings are almost invariably raised near the sea-shore, and are either permanent or temporary, the character of them depending upon the locality, and the material at the workman's disposal. But even those who have fixed dwellings leave them in the summer for tents suited to their migratory habits. In Greenland, the permanent house is built with stones, and turf as a substitute for mortar. It is a low hut, not more than two or three yards high, with a flat roof of wood and turf. It has neither door nor chimney, the use of both being supplied by a vaulted passage, made of stone and earth, sixteen or eighteen feet long, communicating with the middle of the house. The floor is divided into apartments, resembling horse-stalls, by skins reaching from the posts that support the roof to the wall. Each family has its separate room, and each room, in front, a window of seal-skin parchment, which is white and transparent, and the ceiling and walls are lined with the same material. In the room beneath the window, attached to the whole length of the wall, is a deal bench, raised half a yard from the ground, and reserved, as we do best rooms, for visitors. A similar bench is attached to the back wall of the room for the use of the family, the bedding consisting of rein-deer skins. These benches are also used as sofas by day, the women sitting in the rear cross-legged like tailors, and the men in front in the sitting position of civilized life.

In Gilbert Sound, instead of the walls being formed of stone, John Davis informs us they are made of wood; while at Regent Bay, according to Sir John Ross, stone-built houses are used, and the roof, instead of being flat, is arched, and the floor sunk three feet in the earth, a description which exactly answers for the habitations of the Esquimaux of Labrador. From the Coppermine river along the coast westward, and thence to Prince William Sound, the winter houses are built of drift-wood, which is found along the whole route in more or less abundance. At Norton Sound, a sloping roof without any side walls characterises the building, and instead of raised benches, the floor is formed of logs, the entrance being at one end, with a fireplace just within it, and a small hole for the escape of smoke. From Norton Sound to Point Barrow, the houses vary in their construction according to the nature of the ground and the taste of the inhabitants. Some are wholly above ground, some have the roof scarcely raised above it, and others resemble those of the natives of Norton and Prince William Sounds, but they all agree in being constructed with drift-wood covered with peat, and in having the light admitted through a parchment window in the roof.

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[*] Read before the Ethnological Society of London.

They are very comfortable abodes, and now and then of considerable size; one situated between the Mackenzie and Coppermine rivers, was in the interior found to be a square of twenty-seven feet, having the log roof supported on two strong ridge poles, two feet apart, and resting upon four upright posts; the floor, formed of split logs, dressed and laid with great care, was surrounded by a raised border about three feet wide, intended for seats; the walls, three feet wide, were inclined outwards for the convenience of leaning the back against them, and the ascent to the door, which was on the south side, was formed of logs; the outside, covered with earth, had nearly a hemispherical form, and around its base were arranged the skulls of twenty-one whales. There was a square hole in the roof, and the centre log of the floor had a basin-shaped cavity, one foot in diameter, probably intended for a lamp.

The most extraordinary houses are those constructed of the bones of whales, walruses, and other animals. Sir Martin Frobisher first makes mention of these dwellings as existing at Labrador, and Sir Edward Parry and Captain Lyon afterwards found them in use at Melville Peninsula and Igloolik. They are built circular, and of a dome-like form, the lower part or foundation being of stones, and the rest of bones, gradually inclining inwards, and meeting at the top; the crevices, as well as the whole of the outside, are covered with turf, which, with the additional coating of snow in the winter, serves most effectually to exclude the cold air; they are about seventeen or eighteen feet at the base, and about nine feet in height; the entrance is towards the south, and consists of a passage ten feet long, and not more than two in height and breadth, built of flat slabs of stone, and externally covered like the hut; the beds, which are raised, by stones, two feet from the ground, occupy, at the inner end, about one-third of the apartment. Near the huts when they were discovered were large tumuli, formerly dwellings, but then solid moss-covered mounds.

Although during winter the Esquimaux generally occupy permanent dwellings, it not unfrequently happens, from scarcity of provision or some other calamity, that it is necessary for them before spring arrives to seek a new home. When we consider the low temperature of the country, that in many parts it is destitute of wood even for fuel; that the fixed habitations being cemented together by frost cannot be removed, and that the summer tents, from their construction, are not calculated to resist the cold, we are at first led to suppose that, if driven at the inclement season from his accustomed haunts, death must soon close the sufferings of the poor inhabitant of the North Pole. But this is far from being the case, for these ingenious people have learnt to convert snow into building materials, by which means they can raise an establishment for their families in a few hours; an establishment which, from the purity of the material of which it is composed, the elegance of its construction, and the translucency of its walls, gives it an appearance far superior to a marble building. "One may survey it," we are told, "with feelings somewhat akin to those produced by the contemplation of a Grecian temple reared by Phidias; both are triumphs of art inimitable of their kind."*

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* **Franklin's First Journey.**

Having selected a spot where the snow is sufficiently compact, the workmen commence by tracing out a circle of from eight to fifteen feet in diameter, proportioned to the number of occupants the hut is to contain, and then prepare a number of oblong slabs of snow, six inches deep and two feet long, which are tenacious enough to admit of being moved without breaking, or even losing the sharpness of their angles. These slabs, which have a slight degree of curvature corresponding with the circular foundation, are piled upon each other exactly like courses of hewn stone, and care is taken to make them fit closely to each other by running a knife adroitly along the under part and sides, and to cut them so as to give the wall a slight inclination inwards. Tier after tier is thus laid on by one man standing within the wall, who is supplied with material by one or more assistants from without; but for the better convenience of transmitting this supply, when the wall has attained a height of five or six feet, a hole is cut on the south side close to the ground. Thus he continues labouring till he has brought the sides nearly to meet in a perfect and well constructed dome, sometimes nine or ten feet high, which he takes particular care in finishing, by fitting the last block or key-stone very nicely in the centre. The people outside are in the mean time occupied in throwing up snow with the snow-shovel, and stuffing it in where holes have been accidentally left. The builder next proceeds to let himself out by enlarging the hole on the south side in the form of a Gothic arch, intended as a doorway three feet high and two and a half feet wide at the bottom, communicating with which he constructs two passages about twelve feet long and five high, the lowest being that next the hut. The roofs of these passages are sometimes arched, but more generally flat, by slabs laid on horizontally, and the workmen select the building material principally from the spot where the passages are to be made, which purposely brings that part considerably lower than that of the hut.

The work just described completes the walls of a hut, if a single apartment only be required; but if, on account of relationship or from any other cause, several families are to reside under one roof, the passages are made common to all, and the first apartment, in that case made smaller, forms a kind of ante-chamber, from which the entrance is through an arched doorway, five feet high, into the inhabited apartments. When there are three of these, which is generally the case, the whole building, with its adjacent passages, forms a tolerably regular cross. For the admission of light, a round hole is cut on one side of the roof of each apartment, and a circular plate of fresh water ice, three or four inches thick and two feet in diameter, let into it.* The light is soft and pleasant, like that transmitted through ground glass, and is quite sufficient for every purpose. If fresh water ice is not within reach, melted snow is poured into a vessel and thus frozen into a transparent plate.† The next thing is to raise a bank of snow two feet six inches high, around the interior of each apartment, which forms the bed and fire place, the former occupying the sides and the latter the end opposite the door. One might walk completely over them without suspecting the little hive of human beings that is comfortably established below; but this, however, is not always done with impunity when the thawing within has too much weakened the roofs, in which case a leg sometimes makes its way through, to the no small terror of the inmates; for, when these edifices become surrounded by snow drift, it is only by the windows that they can be recognised as human habitations.

The beds are arranged by covering the snow with layers of small stones, of paddles, tent-poles, and pieces of net-work, made of thin slips of whalebone, or twigs of birch and deer-skins, a bed capable of affording not merely comfort but luxurious repose, in spite of the rigour of the climate.

With the lamps lighted and the hut full of people and dogs, a thermometer placed on the net over the fire indicates a temperature of 38° Fahr.; when removed two or three feet from this situation it falls to 32°, and placed close to the wall stands at 23°, the temperature of the open air at the time being 25° below zero. A greater degree of warmth than this, produces extreme inconvenience by the dropping from the roofs, which is prevented by applying a little piece of snow to the place from which the drop proceeds. By melting and freezing alternately, innumerable icicles are formed on the snow walls, which reflect the light like radiant diamonds.* Although this is very beautiful, it is a source of great trouble to the inhabitants, whose lungs become affected from repeated colds and coughs. For this reason, although the houses are formed of snow, coolness is the object always kept in view; and from the inexhaustible building materials always at hand, but little time and labour is required to effect any alterations or additions that may be requisite for the purpose, as exemplified in the instance of a native of Regent Inlet who had closed in his roof within 45 minutes.†

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* Cartwright.

† Ross's Appendix to Second Voyage.

The interior appearance of these habitations is rendered more beautiful when they are situated on the ice, which, being cleared of snow, presents a flooring of that splendid blue, which is perhaps one of the richest colours in nature. If it should happen that the family is increased by births or by the system of adoption in use amongst them, they have to enlarge their buildings, which they effect by adding another apartment, or by building a more roomy house over the old one, and as it were concentric with it; and when completed the old one is removed from within. The natives of the River Clyde were found by Sir Edward Parry inhabiting a hut partly excavated from a bank facing the sea, and the rest built round with stone; a similar hut is figured in Mr Ellis's work; and Captain Cartwright informs us, that the winter habitations of the natives of Labrador are hollowed out of a drift bank of snow in the form of an oven.

Equal in beauty to the snow-house is that constructed of fresh-water ice. The material is collected in large transparent slabs, arranged in an octagonal form, and the roof formed of walrus skin or snow. These dwellings are so transparent that even at some paces distance it is possible to distinguish those who stand within them; yet they are so completely airtight as to be perfectly warm.‡

In regard to dress the Esquimaux, in design and execution, may vie with the world, and the civilized traveller would do well to take a lesson from these "children of the chase," for the most accomplished furrier sinks insignificantly before them, so tastefully are the various coloured skins of the country worked by them into articles of apparel. Thus the dark and mottled fur of the seal is placed in the centre of the back or breast of the dress, while the lighter shades are on the sides; in the sleeves the fore and back piece are often of two colours, and the cuffs of a different hue, each of the numerous varieties of seal affording a peculiar skin, which enables them to arrange the most opposite and glossy colours in the same habit; and at the parts of the body where it is necessary to have protection from cold there are no seams, therefore no crannies for the entry of the weather. In order, however, to unfold and fully appreciate the ingenuity of the people in this respect, the dress must be turned inside out, and personally inspected.

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* *Cartwright.* † Ross. ‡ Lyon.

The coats are of three kinds, one for the winter, one for the summer, and one as an over-all in wet weather, and in shape somewhat resemble a shirt without its front opening, with a large hood attached, which is drawn over the head at pleasure. This is bordered with the white fur of the deer, and thus presents a lively contrast to the dark face which it encircles. The front or belly part of the coat is cut off square with the upper part of the legs; but behind, it is formed into a broad skirt, rounded at the lower end, which reaches to within a few inches of the ground. Many of the jackets, and particularly those of the females, are lined with the skins of birds.* The lower edges and tails of the dresses are in some cases bordered with bands of fur of an opposite colour to the body, and it is a favourite ornament to hang a fringe of little strips of skin beneath the border, which has a very pleasing appearance. The hood of the coat worn by the women is comparatively of immense size, which, as well as a covering for the head, answers the purpose of an infant's cradle for two or three years. At the shoulders is a wide bag-like space to afford room for the child, and to facilitate its removal from the hood to the breast without taking it out of the jacket,† and in front, instead of being square, it terminates in a rounded projection which extends about half way down the leg. The difference between the summer and winter coat generally consists in the one being formed of seal and the latter of rein-deer skin; but every variety of fur is occasionally worn; for instance, at Prince William Sound, the natives were found by Captain Cook wearing sea-otter, fox, racoon, martin, seal, and water-fowl skins, the former being most common. At Schismareff Inlet, the skin of the rein-deer and dog was the prevailing material; at Regent Inlet, that of the Polar bear; at Regent Bay, both of the bear and the dog; and at Melville Peninsula, of the rein-deer only.

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* Kotzebue.

† Parry.

The over-all coat termed Kamlaika, which answers most effectually the purposes of a "Mackintosh," is very ingeniously made of seal or walrus-gut, or parchment skin cut into strips of an inch broad, and most skilfully sewed together. It effectually protects the fur clothes from rain or sea-spray. According to Egede, it is worn by the natives of Greenland between two jackets; and Ellis, by the inhabitants of the Savage Islands next the skin. The breeches of fur, tied tightly round the loins, reach a little below the knee, and hang loosely over the boots, which in the feet are moccasin-fashioned; both articles of dress being composed of rein-deer skin in winter, and seal-skin in summer,—the latter soled with the tough hide of the walrus, to protect them from the rough ground. As with the jacket, so with the boots worn by the women; they are made to suit the wearers' particular occupations, and are, without dispute, the most extraordinary part of their equipment. They are so large as to resemble leather sacks, and give a most deformed, and, at the same time, ludicrous appearance to the whole figure. The bulky part is at the knee, and the upper end is formed into a pointed extremity, which covers the front of the leg, and is secured by a button or knot within the waistband.* These capacious pouches are used as pockets in the ordinary sense, and as temporary beds for infants; and, when in the vicinity of white men, as receptacles for stolen goods. The mittens in common use are of deer-skin, with the hair inside; but, in fact, every kind of skin is appropriated to this necessary part of the winter gear. The natives of Behring Straits skin the paw of the bear, and wear it as a glove.

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* Ellis.

Besides the differences I have mentioned between individuals of the same tribe, there are others between tribe and tribe. At the River Clyde and Regent Bay, the man's jacket behind is quite straight, while before there is a sort of scollop in the centre. To the westward of Mackenzie River, a skirt is worn before as well as behind; and at the Coppermine River, the tail of the woman's jacket is not more than a foot long.* A wooden cap, carved to resemble the bear or the seal, or, in fact, any animal of which they may be in search, is worn to the westward of the Mackenzie, in order to facilitate their approach to within gun-shot.† A cap made of the skin of the tail of the buffalo is worn in the summer to keep off the musquitoes.‡

At Labrador, the women's boots, instead of being looped up to the breeches, are "holden," says Sir Martin Frobisher, "from falling down about their feet by a piece of bone placed within them for their whole length;"§ according to Mr Henry Ellis, they have an additional piece of whalebone to keep them wide at the top; and at Lawrence Island and the river Clyde, they are worn close to the leg like those of the men. The Esquimaux of Prince William Sound dispense with the hood to the jacket, and wear a high truncated conic cap made of straw, and sometimes of wood. At the Savage Islands, Southampton Island, the River Clyde, and occasionally Prince William Sound, the women, instead of breeches, wear little thigh wrappers, which form but a very imperfect covering, and is the cause of their frequently getting frost-bitten; yet they are so wedded to custom, that they will not add one inch to the established length. A tippet covering the shoulders, cut from the white part of the deer-skin, is occasionally worn by some of the younger persons as an ornament. The girdle worn round the waist is frequently ornamented with the bones, teeth, or nose of the fox or wolverine, or the ears of the rein-deer, hanging in pairs to the number of twenty or thirty, trophies of the skill of the hunter to whom the wearer is allied, and the same ornaments are attached as a fringe to the lower parts of the jackets. To the drawing-strings of the women's breeches, which are of greater length than those of the men, and made to hang down on one side, are frequently appended as ornaments, a grinder or two of the musk-ox, a piece of carved ivory, a small ball of wood, or perforated stone. At Schismareff Inlet, the men ornament the ends of these strings with a tuft of hair, the wing of a bird, or the tail of a fox; and as they do not take the precaution to turn the string on one side, as is usually the case, it dangles behind as they walk, and gives them a very ridiculous appearance. Captain Beechy thinks it probable, that it was this feature that gave rise to the report of the Tschuktchi of Northern Asia, recorded by Müller, that the people of America had tails like dogs.

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* Cook. † Sauer. ‡ Ellis. § Frobisher.

The tent of the Esquimaux is merely a temporary summer habitation, formed generally of the skin of the walrus, cut into layers of about the thickness of a dollar, and extremely transparent and oily. The entrance or front faces the south, and as a protection to its back or northern point, seal-skins are placed. Beneath this is the sleeping place, alone distinguished by having several deer-skins spread on the bare ground. The seams of the covering are sewed together so as to be perfectly water-proof, and to the door-way are hung additional skins. One central pole of bones or broken spears, tacked together, gives height and support, and large stones or gravel, instead of pegs, size and shape to the tent; the diameter of the floor being ten, twelve, or fourteen feet, according to the number of inmates. From the top of the pole one or two skin lines are carried for security and steadiness, which, at Kotzebue Sound and Labrador, is rendered unnecessary; for in these localities, four poles are used, which, uniting at the top, and spreading at the base, give strength and shape to the tent.

The drawings made by Mr Henry Ellis and Dr Richardson of the tents observed by them at Hudson Bay and the

Polar Sea, agree with those of Kotzebue Sound* and Labrador,† except in the projection of the poles above the skins. Sir Edward Parry states this to be the fact at the River Clyde; and we may infer, therefore, that Mr Henry Ellis and Dr Richardson are right, although they have made no allusion to this peculiarity in their respective narratives.

Egede and his commentator Crantz inform us, that the natives of Greenland, who cover their tents wholly with sealskin, form the entry with seal-gut, which, from its transparency, answers the purpose of a window, a contrivance unnecessary with the highly translucent walrus skin. The tents sometimes contain two families, if they are related, or a double tent is formed by joining the mouths of two single ones, and making the opening on one side. Sir John Franklin discovered a tent to the westward of the Mackenzie, capable of holding forty persons, supposed to have been a trading establishment.

Besides the sleeping place, there is but little standing room in the tent; as on one side is the larder,—an accumulation of flesh, blubber, bones, birds, eggs, &c., which lie at the mercy of the heels of all that enter; but the careful tread of the inmates enables them to avoid that which the stranger in the land finds a difficult task. Sir Edward Parry's party were not so sure-footed, and the juices of these arctic luxuries, we are told, "formed an intolerable filthy mud on the floor;" and thus the Esquimaux had good reason to complain of their *soi-disant* more civilized brothers, whose awkwardness not only destroyed their carpet of earth, but deprived them of a portion of their very scanty winter store.

There are two kinds of boats in use, the caiak or man's boat, and the oomiak or woman's boat, which are admirably adapted to meet the circumstances in which they are placed. Division of labour is not, nor ever likely to be, established among this isolated family; every man, therefore, is his own boatbuilder; and it is no mean test of intelligence, to find it admitted by all, that the most practised civilized artisan could not possibly surpass them, either for symmetry or execution. The caiak was first described by William Baffin, who, while a youth, wrote the Account of James Hall's Voyage of Discovery in 1607, and whose subsequent career entitled him to have a very considerable bay of the Atlantic named after him. The length is from 16 to 20 feet, and the breadth at the centre from a foot and a-half to 2 feet, and the depth about 1 foot, the head and stern gradually inclining to a point from the centre; it has, therefore, been very justly compared, in shape, to a weaver's shuttle. The bottom is rounded, and has no keel. Twenty-two little beams or cross pieces keep the frame on a stretch above, and two strong battens run, one from the stem and the other from the stern, towards the centre, where they are attached to a hoop of bone or wood, of a sufficient size to admit the body. The frame is entirely covered, with the exception of a circular hole in the centre, with fresh dressed seal or walrus parchment, and, when complete, it weighs about sixty pounds, which, by the form of the rim, can be carried on the head without the assistance of the hands.

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* Kotzebue.

† Frobisher.

There is a difference in the form of the caiak, according to the *locale* of the people, which lies in the elevation and shape of the rim, and in the greater or lesser curvature of the extremities. Throughout Esquimaux land, comfort *versus* custom is the motto of some few of the inhabitants in relation to the rim, and instead of being of a uniform height, it is raised at the back, the better to support the trunk of the body. At Prince William Sound, according to Captain Cook, the bow curves somewhat like the head of a violin, a peculiarity of form which answers for one figured by Mr Henry Ellis as in use at Hudson Bay. This is, however, doubtful, for in Captain Cook's caiak we have the letterpress, and in that of Mr Henry Ellis the figure alone to guide us. The caiak of Greenland has a knot of ivory at each end, to protect the sharp point of the extremity; it is possible, therefore, that the curvature described by Cook and Ellis had a protective use.

At Prince William Sound the caiak is frequently built with two,[*] and even three,[†] circular openings, so as to hold two or three men, in which instances a single, instead of the usual double paddle is used; and it is worthy of remark, that Thorwold, who fell mortally wounded on the eastern coast of Greenland, in 1003, whilst leading a most uncalled for and brutal attack on this peaceful nation, states that the boats there held three persons. If this fashion was at the time prevalent, which is likely from the psychological history of the people, it was not so in 1576.[‡] Necessity frequently obliges the Esquimaux to lash together various little scraps of wood, ivory, and bone, for paddles,[§] but when wood is at hand they are well formed, and the broad blades neatly shod with bone, to enable them to bear the concussion of the ice.

The oomiak, called baidar at Ochotsk and Kamtschatka, is from 20 to 25 feet long, by 8 broad, and is capable of accommodating twenty persons. At Hudson Straits and Greenland it is navigated by women, and at the north-west corner of America by men, the women occasionally assisting. The one is of superior workmanship, and is propelled by men with single paddles, the rowers facing the bow; the other by women with rudely shaped oars, resembling a spade at the water end, the rowers facing the stern. These family boats all agree in the general framework, and in being covered with seal or walrus skin, but they vary in form. They are not in use at Regent Inlet and Melville Peninsula. The oomiak of Hudson Straits and Greenland is flat-sided and flat-bottomed, about three feet high, and nearly square at the bow and stern, and contains from five to six seats secured to the gunwales by thongs, and that of the north-west coast is sometimes pointed at the bow and stern, and at other times at the bow only. The sail of the oomiak is what sailors call lug-shaped, and is formed of walrus gut sewed together with great neatness, in breadths of about four inches, and weighs only three pounds and three quarters. The mast, which is placed well forward, is made of wood, and has a very neatly formed ivory sheave for the halyards to run on. Considerable taste and attention is bestowed on the bow and stern of the oomiak; but the Esquimaux more particularly prides himself in the neat appearance of his caiak, and has a warm skin placed in its bottom beneath the rim for a seat. The dexterity with which it is rowed, the velocity of its way, and the extreme elegance of its form, renders an Esquimaux, when sitting independently, and urging his course towards his prey, an object of the highest interest; and it is really wonderful that in so frail a bark he can defy the raging storm, and give battle to the polar bear and the mighty monsters of the deep.

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[*] Cook. [†] Sauer. [‡] Parry. [§] Frobisher.

Captain Cook has informed us, "that every thing the Esquimaux have, is as well and ingeniously made as if they were furnished with the most complete tool-chest; and that their sewing, plaiting of sinews and small work on their little bags, may be put in competition with the most delicate manufactures found in any part of the known world. In short, it appears that their invention and dexterity in all manual works, is at least equal to that of any other nation." Let us see what are the tools they work with, and proceed to weigh the opinion Captain Cook has so strongly expressed. A hand chisel made of stone, copper, or ivory, of five or six inches long and about two broad, bevelled away at one end, and fixed in wood or bone handles, is in pretty general use.* This primitive mortise chisel is frequently lashed to the end of a piece of wood about twelve or fourteen inches long, in such a manner as to act like an adze,† and made almost after the manner of that of Otaheite,‡ which from its beauty finds a place in the drawing-room of the rich. Captain Lyon found a hatchet of English manufacture among the Esquimaux of Melville Peninsula thus mounted, and the shaft was scooped out in such a way as to allow the hand to fit it, each finger having its particular place. The knives used by the men are of two kinds; the one is made of the tusk of the walrus, cut or ground sufficiently thin for the purpose, and as it retains the original curve of the tusk, it resembles the little swords which the English child plays with as a toy; the other is of copper,‖ or iron,§ triangular in form, and is let into a handle of wood or horn a foot long, to which it is secured by rivets or thongs. A knife of this description, found by Hearne at the mouth of the Coppermine River, was made of copper and heart-shaped, while one discovered by Sir Edward Parry at Melville Peninsula, was formed of iron and triangular. The gallant officer is of opinion, that the Esquimaux are probably indebted to an indirect communication with our factories in Hudson Bay for this form. The fact, however, of Hearne having found the same kind of knife at the Coppermine River, of which Sir Edward Parry was not perhaps aware, would appear to establish it as an invention of their own. The woman's knife exactly resembles that used by our cheesemongers, which renders a particular description of it unnecessary. A sort of saw, used instead of a knife for cutting blubber, was found on the eastern coast of Greenland, by Captain Graah; it consisted of a lance-formed piece of fir, along the edges of which were inserted rows of sharks' teeth, which were secured by small nails of bone. This instrument, he adds, was formerly made use of on the west coast. Thus much of the contents of an Esquimaux tool-chest.

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* Richardson, Beechy, Hearne.

† Hearne.

‡ Cook.

‖ Hearne.

§ Parry.

Fotherby, in 1615, was the first to make mention of the Esquimaux sledge, and that it was "shod or lined with bones," and Captain Luke Fox, in 1631, was the first to describe it; but we are indebted to Captain Cartwright, in 1770, for entering minutely into its construction. "The sleds the Esquimaux make use of," says Captain Cartwright, "are made of two spruce planks, each 21 feet long, 14 inches broad, and 2 inches thick, which are hewn out of separate trees (because they are not acquainted with the use of the pit-saw); they are placed collaterally with the upper edges at the distance of about a foot asunder; but the under edges are somewhat more, and secured in that position by a batten 2 inches square, which is placed close under the upper edges. The fore-ends are sloped off from the bottom upwards, that they may rise over any inequality in the road. Boards of 18 inches long are set at the upper edges of the sled, 3 inches asunder, to place the goods upon, and to accommodate the driver and others with a seat. The under edges are shod with the jawbone of a whale, cut into lengths of 2 or 3 feet, half an inch thick, and are fastened on with pegs of the same. This shoeing is durable, and makes them slide very glibly. The wood work is sewed together with split whalebone. A couple of holes are bored through the fore ends of each plank, in which are inserted the two ends of a strong short thong, made out of the hide of a sea cow, and secured by a knot, and to the middle part of the thong a separate one is fastened from each dog."

In 1631, wood must have been more plentiful than in Captain Cartwright's time; for Captain Luke Fox not only represents the side boards of the sledges to have been 4 inches thick, but informs us that, by robbing the graves, which were roofed with the sledges of the departed, after Indian custom, he obtained a boat-load of firewood in a single sacrilegious act. At the River Clyde, the sledges are made altogether of bone, the right and left jaw-bones of a young whale forming its sides, and the ribs of the animal the cross pieces, and for the back are placed rein-deer horns in an upright position.* Bone sledges are also in use at Schismareff Inlet† and Regent Bay.‡ At Regent Inlet the sledge is formed of a "number of salmon, packed together in the form of a cylinder about 7 feet long, and wrapped up in the skins taken from the canoes, which now cease to be of use; when well corded with thongs, two of these cylinders are pressed into the shape of the runners, and having been left to freeze, are secured by cross bars made of the legs of the deer or musk-ox, so as to form the bottom of the sledge; the bottom of the runner is then covered with a mixture of moss, earth, and water, which soon freezes to the depth of 2 inches, after which comes the final process of plaiting the surface. The operator takes some water in his mouth, and when somewhat mixed with saliva, it is deposited on a bear skin, which is then gradually rubbed over the runner as by a brush, till a coating of half an inch thick is produced, which has a more than usual degree of tenacity, and is more slippery than the ordinary material. These carriages travel much more lightly than those shod with iron; but as they cease to be of use as soon as the thermometer rises above the freezing point, they are taken to pieces, the fish being eaten, and the skins converted into bags, while the bones are reserved for the dogs."* In every instance the sledge is shod with pieces of hard bone which can be replaced when worn out.

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* Parry. † Kotzebue. ‡ Ross.

The skin of the walrus, during the coldest part of the winter, is often manufactured into runners, for, when hard frozen, it resembles an inch board, with ten times the strength. Another ingenious contrivance is, the casing of moss and earth in seal skin, so that, by pouring a little water, a round hard bolster is easily formed. Across this kind of runner, there is the same arrangement of bones and sticks on the upper part, and the surface which passes over the snow is coated with ice, by mixing snow with fresh water. A rough piece of walrus hide, shaped like a tray or a flat slab of ice, hollowed like a bowl, for trifling purposes, is frequently used.†

In a country where the pliant twig holds no place, and where frequently driftwood is so scarce as to leave it destitute of that important article even for fuel, it is indeed an interesting inquiry to determine the means by which the Esquimaux has supplied himself with a bow. When formed of one piece of wood, the bow is of the ordinary make, strengthened on the back with 100 or more of small plaited or twisted sinews,‡ which, to use the words of Sir Martin Frobisher, are "not glued to, but fast girded and tied on." The Esquimaux of Behring Straits bestow much care in giving the bow the proper form, and for this purpose they wrap it in shavings soaked in water, and hold it over a fire for a time. It is then pegged to the earth in the form required. By the assistance of the sinews at the back, the bow preserves its elastic power; and, by slackening or tightening them, it is rendered weak enough for the child, or strong enough for the most powerful man;§ and when "fast girded," it causes the implement, when unstrung, to turn the wrong way. They have also the power of altering the length of the bowstring to their pleasure, by twisting the several strings, often of fifteen or twenty plaits, of which it is composed.

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* Ross. † Parry, p. 206. ‡ Egede, Parry, Lyon, Franklin, Beechy. § Cartwright.

Two varieties of form and construction have been found amongst the natives of Hudson Straits. The one has been described and figured by Mr Henry Ellis; and a specimen of the other is in my possession. Sir Edward Parry did not meet with either of these bows among the natives of Melville Peninsula, although close neighbours to those of Hudson Straits. The bow met with by Mr Henry Ellis, "was made of three pieces of wood, each forming a part of the same arch, very nicely and exactly joined together." The bow in my possession differs from that of Mr Henry Ellis, in being flat instead of arched in the centre, and having a small piece of wood bound with fish-skin to the back part of the junction of the two curved extremities with the central flat portion, and is composed of five pieces of wood. At Melville Peninsula, for the want of wood, the horn of the musk ox, thinned horns of deer, and other bony substances are used. To protect the wrist from the abrasion which would ensue from frequent use, the Esquimaux of Behring Straits buckle on a piece of ivory about three or four inches long, hollowed out to the wrist, or a guard made of several pieces of ivory or wood fastened together like an iron-holder. The bow-string is, moreover, in contact with about a foot of the wood at each end, and, when used, makes a report fatal to secrecy; their more warlike neighbours are careful to muffle these parts with fur.* To make the bow draw stiffly, they dip it in water, which causes its back and string to contract, and creates a greater impetus. Sir Martin Frobisher informs us, that the bows are three feet long; Captain Lyon adds six inches to that length; and the one in my possession measures five feet three inches. It is used in the horizontal position. Lieutenant Edward Chappel must have been the dupe of some Israelitish Esquimaux, or he would scarcely have pronounced "the whole fabrication of the bows and arrows not to surpass the workmanship of an English schoolboy." Specimens of the industrial arts of the Esquimaux were presented by Lieutenant Chappel to the Library of the University of Cambridge.

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* Beechy.

The arrows are short, light, and formed without regard to length or thickness; "they are three pieces, nocked with bone and ended with bone, with those two ends and the wood in the midst; they pass not in length half a yard, or little more. They are feathered with two feathers, the pen end being cut away, and the feathers laid upon the arrow with the broad side to the wood, insomuch that they seem, when they are tied on, to have four feathers. They have also three sorts of heads to their arrows; one sort of stone or iron proportioned like a heart; the second sort of bone, much like unto a stopped head, with a hook on the same; the third sort of bone, likewise made sharp on both sides, and sharp pointed. They are not made very fast, but lightly tied, or else tied in a nocke, that upon small occasions the arrows leave these heads behind them, and they are of small force except they be very near when they shoot."* The bow, when of the ordinary make, and a few arrows, are carried in a neatly formed seal-skin case; and attached to the side, in a little bag, is a stone for sharpening, and some spare arrow-heads carefully wrapped up.

Their spears are of various kinds, the difference chiefly consisting in material, rather than form. The *kā-tĕ-lēēk*, with which the whale and walrus are attacked, is a large and strong handled spear, especially towards the middle, where there is a small shoulder of ivory securely lashed to it for the thumb to rest against, and thus to give additional force in throwing or thrusting it. The ivory point of this weapon is made to fit into a socket at the end of the staff, where it is secured by double thongs, in such a manner as steadily to retain its position when a strain is put upon it in the direction of its length, but immediately disengaging itself with a sort of spring, when any lateral strain endangers its breaking. This weapon, so far, displays little art or ingenuity; but an appendage called *siātkŏ*, consisting of a piece of bone three inches long, and having a point of iron at one end, and at the other a small hole or socket, to receive the point of the *kā-tĕ-lēēk*, is a masterpiece of art. Through the middle of this instrument is secured the *āllek*, or line of thong, of which every man has, when sealing, a couple of coils, each from four to six fathoms long, hanging at his back. These are made of the skin of the ozuke as in Greenland, and are admirably adapted to the purpose, both on account of their strength, and the property which they possess of preserving their pliability even in the most intense frost. To the end of the *āllek*, when the animal pursued is in open water, they attach a whole seal-skin, inflated like a bladder, for the purpose of tiring it out in its progress through the water. The *akliak*, a lighter kind than the former, also ivory-headed, with the siatko attached, is used for the large seal, and has a bladder fastened to it, and a loose head with a line attached; this being darted into an animal, is instantly liberated from the handle which gives the impetus. The *oonak* used for the small seal is of the same make as the former, but wants the bladder.

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* Frobisher.

When a seal is seen, the siatko is taken from a little leathern case, in which, when out of use, it is carefully inclosed, and attached by its socket to the point of the spear; in this situation it is retained by bringing the āllek tight down, and fastening it round the middle of the staff by a slipknot, which may instantly be disengaged by pulling on the other end of the line. As soon as the spear has been thrown and the animal struck, the siatko is thus purposely separated, and being slung by the middle, now performs very effectually the important office of a barb, by turning at right angles to the direction in which it has entered the orifice. This device is, in its principle, acknowledged to be superior even to our barb; for the instant any strain is put upon the line, it opposes its length to a wound only as wide as its own breadth.

The nuguit, for striking birds, young animals, or fish, has two forms; the first has two prongs like a fork at the extremity, and three other barbed ones in the middle, diverging in different directions, so that if the end pair should miss, some of the centre ones should strike. The second has no diverging prongs in the middle, but three instead of two at the extremity, and may be termed, by way of distinction, the trident spear. Sir John Ross, in his account of the

Esquimaux of Regent Inlet, describes the trident spear, "because it was different from any of which he had read." Now Sir Martin Frobisher has accurately described this spear, comparing it to "our toasting irons, but longer," as well as Captain Lyon; and it is difficult to understand that the latter gallant officer's admirable description could have escaped Sir John Ross's attention. When employed for striking birds, to give these spears additional velocity, a throwing-stick is used, which is constructed of a flat board of about eighteen inches long, having a groove to receive the staff, two others, and a hole for the fingers and thumb, and a small spike fitted for a hole in the end of the staff. Mr Henry Ellis has figured this singular instrument, and specimens of it were brought home by Captain Billings and Lieutenant Chappell.

The sling is in use amongst the natives of Labrador,* Hudson Straits,† and the Great Fish River Estuary,‡ and is very destructive in the hands of an Esquimaux, who directs it with great force and unerring aim. Amongst the minor implements of the chase are, their fish-hooks, consisting only of a nail, crooked and pointed at one end, the other being let into a piece of ivory to which the line is attached; a piece of deer's horn or curved bone, only a foot long, used as a rod; a long bone-feeler for plumbing any cracks in the ice through which seals are suspected of breathing, and also for trying the safety of the road; as a float, a most delicate little rod of bone or ivory, of the thickness of a fine knitting needle, and about a foot long, having at the lower end a small knob, and at the upper a fine piece of sinew tied to it, so as to fasten it loosely to the side of the hole; small ivory pegs or pins to stop the holes made by the spears in the animal's body, in order that the blood, a great luxury to the natives, may be saved; and an instrument, shaped something like a shoe-horn, with four holes at the small end, communicating with a trough that extends along the middle, and widens as it nears the broad part. This is used to procure blood from the dying animal, by inserting the end with the holes into the wound, and placing the mouth at the opposite end of the trough, to receive the liquid as it flows.

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* Frobisher. † Ellis. ‡ King.

In order to limit their observation of distant objects, and to protect themselves from snow-blindness, occasioned by the reflection of the sun's rays from off the ice-flakes of snow, they wear an eye-shade of wood or ivory, or of both combined. On the inside it is hollowed to receive the bridge of the nose, on which it rests in front, and to give free play to the eyelids, while on the outside it is convex, and longitudinally and very narrowly divided for the purposes of vision. Two strings are attached to it to confine it in its place.

And in order to protect themselves from the Russian traders, the natives of Prince William Sound and Schismareff Inlet, wear underneath their dress a jacket or coat of mail* made of thin laths, bound together with sinews, which make it quite flexible, though so close as not to admit an arrow or dart. It only covers the trunk of the body, and has been compared by Captain Cook to the stays of civilized life. According to Sauer, it has a flap before, reaching down their thighs, but so made as to rise or fall, and permit their sitting in their oomiaks: a similar flap hangs on the breast, which may be raised as high as the eyes. Straps fasten this armour on the shoulders, and strings tie it round the body on one side. At Schismareff Inlet, instead of wood, their coat of mail is made of eider drake skin.†

They have an ingenious contrivance for detaching fleas from the back, or such parts of the body as the hands cannot reach. This is a rib of a seal, having a bunch of the whitest hair of the deer attached to one end, and on thus rubbing the places which require it, the little animals stick to it: from their colour they are easily detected.

Almost all the uncivilized races have some notion of drawing, generally a representation of themselves, of the animals of the chase, and the implements used in their capture. Captain Beechy has described a picture drawn on one of their implements of the chase, by one of the natives of Kotzebue's

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* Cook, Sauer.

† Beechy.

Sound, which represented in one part a hunter in pursuit of a herd of deer, in a stooping posture on snow-shoes; in another his nearer approach to his game, and in the act of drawing his bow; in a third, the act of throwing his spear at a seal, with an inflated skin as a decoy: the animal was placed upon the ice, the man lying on his belly, with a harpoon ready to strike it; in a fourth, the dragging a seal home upon a small sledge, and several oomiaks busy in harpooning whales which had been previously shot with arrows; and thus, by comparing one with another, a complete insight into their habits was elicited. On the eastern coast of Greenland, Kanick, a painter of repute among his colleagues, ornamented the walls of his house with figures cut out of black skins representing seals, walruses, bears, and Greenlanders, "in which menagerie," says Captain Graah, of the Danish Navy, who discovered them, "he soon recognised himself."* Captain Parry informs us, that Toolemak, a native of Melville Peninsula, learned himself to draw very fairly, by copying prints; and Ayokitt's productions were so curious and ingenious, as "to determine Captain Lyon on treasuring them." In the animals there was one striking peculiarity, which consisted in having both eyes on the same side of the head. Sacheuse, the interpreter of Sir John Ross's first expedition, acquired the art to an extent which enabled him to make a drawing of the first interview of the exploring party with the natives of Regent Bay a highly ludicrous scene, which, from its value, was engraved as an illustration to the Narrative of that Expedition. Sir John Franklin's interpreter, Augustus, was accustomed to spend whole days in looking over a colleclection of portraits, amongst which was one of the Duchess of Kent, the mother of our Sovereign. The representation so won his heart, that in the list of presents which he furnished to be forwarded to him from England, was that of "a wife like the Duchess of Kent."

The art of carving is universally practised,—its state of perfection keeping pace with our progress, and along their migration from east to west. Carvings in imitation of the human form were found by Captain Graah on the eastern coast of Greenland of a very rude character.* We no sooner, however, reach the eastern coast of America, than we find models of men, women, and children, of birds, beasts, and fishes, and of every kind of implement and culinary utensil, executed in a masterly style. An Esquimaux woman, without her dress, obtained from the Esquimaux, shews a proficiency in anatomy as complete as the learned professor of the Royal Academy could possibly wish his most accomplished pupils to attain. Also, one with the dress on shews, that they can chisel the drapery with equal truth. "An ivory fish," purchased by Captain Lyon, "neatly formed, and about three inches long, had the eyes made of small lumps of iron pyrites."† Ivory chains, most ingeniously cut out of solid pieces of ivory, were found in the possession of the Esquimaux of Behring's Straits, each link being separately relieved, and sometimes twenty-six inches in length.‡ For what purpose they were used, Captain Beechy knew not; but part of the last link was frequently left solid, and formed in imitation of a whale; "and these chains being strong," he adds, "may in some way or other be appropriated to the capture of that animal." Men and women are generally dressed in clothes, which are made with seams and edgings precisely similar to those in daily use. The ivory of which they form their models is cut by continued chopping with a knife, one end of the ivory resting on a soft stone, which serves as a block. To smooth and polish the work when finished, a gritty stone is used as a file, and kept constantly wetted with saliva.

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* Graah, 131.

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* Graah, 99.

† Lyon, 283.

‡ Beechy.

Thirteenth Letter on Glaciers. Addressed to Professor Jameson. By Professor J. D. FORBES.*

Acceleration of the Surface Motion confirmed—New Determinations of the Velocity of different Glaciers—Discovery of a Knapsack buried ten years in the Ice—Manner of Conversion of the Névé into Ice—Manner of ejection of Stones from Glaciers.

MY DEAR SIR,—Since the completion of my Twelfth Letter, I have observed, in the *Comptes Rendus* for 26th October, an account of a communication made to the Academy of Sciences by M. Martins, of an experiment on the relative motion of the surface and inferior part of a glacier, on which I have also made the experiments detailed in my Eleventh Letter (dated 16th September), and published on 1st October in The New Edinburgh Philosophical Journal. My experiments, it will be recollected, were made at three points of the *terminal face* of the Glacier des Bois, and proved, as I had long ago anticipated, that the superficial ice has by much the most rapid motion. MM. Dollfus and Martins arrive at the same result, establishing the desired identity with the motion of rivers ; but their experiment being made, not on the *terminal face*, but only on the steep *lateral face* of a glacier, is open to the possible objections which I anticipated in my former letter, and which I carefully endeavoured to avoid (see p. 418 of the Eleventh Letter, and fig. 2 of the accompanying Plate), so that even the most scrupulous might be, if possible, satisfied. It is pleasing to me to find that the French observers corroborate my result ; but I must remark, that they obtained it a fortnight later, and my publication preceded theirs nearly a month.

I shall not seem to insist too much upon the conclusiveness of this observation, when it is recollected that an opponent of the Viscous Theory has virtually staked the question of the cause of glacier motion upon such an experiment (notwithstanding that I think he has attached to it an undue importance), in the following passage :—

" * * The claims of the two theories (the sliding theory

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* Read before the Royal Society of Edinburgh, 21st December 1846.

" and that of plastic motion) will undoubtedly be determined " by other means. The observations required are such as " shall determine, as far as possible, the motions of the upper " and lower surfaces of a glacier. We may never hope to " have access to the bottom of a glacier in its deeper portions, " but at the extremities of glaciers the amount of sliding " may easily be ascertained, as well as at many other points, " probably, if sought for, along their flanks: fissures, also, of " considerable depth are not unfrequently met with, in which " the deviation from verticality, if it exist, might be easily " determined; and though the evidence thus obtained might " not afford positive demonstration with respect to the deepest " portions of a glacier, still, should it all concur in shewing " an approximate equality in the motions of the upper and " lower surfaces, every candid and impartial mind must admit, " I conceive, the *sliding* in preference to the *viscous* theory; " but if, on the contrary, it should be proved that the velocity " of the upper bears a large ratio to that of the lower surface, " the claims of the latter theory must be at once admitted."* It is very fortunate that independent observers on different glaciers should have arrived, in ignorance of each others' results, at conclusions which permit only the alternative favourable to the viscous theory to be adopted; the observations having been made, too, one at "the extremity," the other "along the flanks," as suggested in this passage.

I shall now proceed to conclude the account of the most material observations which I had an opportunity of making last summer: and those which remain refer chiefly to three points: (1.) The rate of motion of glaciers generally, and especially during the summer of 1846; and, (2.) To the conversion of the *Névé* into the icy condition; (3.) As to the apparent ejection of stones upon the surface.

(1.) RATES OF MOTION.

A. *At points previously observed.*

It is very interesting to compare the ANNUAL motion of glaciers in different years, and also the comparative motions of a given point, at the same season, in different years. *I* have obtained several such results on the Mer de Glace of Chamouni, all of which tend to shew a marked increase of the rates of motion during the two years which have elapsed since my last measures were made.

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* Mr Hopkins, in *London Philosophical Magazine*, March 1845, p. 250.

Thus, at station C, the *Pierre Platte*, opposite to the promontory of Tacul,* from the 19th August 1844, to 21st July 1846 (701 days), the advance was 622 feet, or 10·65 inches daily.

The following little Table shews the relation of this to former years.

	1842–3.	1843–4.	1844–6.
Daily Motion in Inches,	9·47	8·56	10·65
Annual Motion in Feet,	288·3	260·4	323·8

The mean velocity was, therefore, about a fourth part greater in 1845–6 than in 1843–4.

The following Table shews the daily motion, in inches, of a series of transverse stations nearly opposite to the Montanvert, and marked with the numbers D 2, D 5, D 6, D 3, since the commencement of my observations.† The first in order was about 100 yards from the western shore, D 5 was 130 yards farther, D 6 was 75 yards farther, and on the axis of the "dirt-bands:" D 3 was 60 yards farther, near the medial moraine, and about 350 yards from the eastern side of the glacier.

Mean Daily Motions.

	D 2.	D 5.	D 6.	D 3.
	Inch.	Inch.	Inch.	Inch.
1842, Aug. 1–9, . .	16·6	24·7		
1843, Sept. 11–13, .	16·0			
1844, Aug. 17–22, .	17·4	23·3	23·7	23·9
1846, July 22—Aug. 3,	18·7	25·4	26·8	26·5‡

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* See Travels in the Alps, 2d Edit., p. 92 and 135, and Map annexed.

† Travels, 2d ed., pp. 137–140.

‡ From 25th July to 3d August only.

Relative Daily Motions, D 2 *being* = 1.

	D 2.	D 5.	D 6.	D 3.
1842,	1·000	1·332	1·356	1·367
1844,	1·000	1·339	1·362	1·374
1846,	1·000	1·404	1·516	1·486

It will be seen that both the absolute and relative motions were markedly greater in 1846 than in the previous years; and that this also agrees with the annual motions at station C given above.

A block, on the ice opposite Les Ponts (between Montanvert and the Angle), marked with the letter P on the 18th September 1843, moved 486 feet from that date to 9th August 1844 (or 331 days), being 17·62 inches daily, or 536 feet annually. The same block re-examined on the 16th July 1846, had moved 776 feet since the 9th August 1844, or only 13·2 inches daily, or 402 feet annually. The retardation must be attributed to the block having been thrown almost upon the moraine, as happened to the block D 7 under similar circumstances.* To avoid such a mischance again, I marked the position of a fine block now near the centre of the Mer de Glace opposite to *Les Ponts*. It was painted with the letter V on both sides, and its position determined with reference to a point on the pathway between the two *Ponts*, to which it was exactly opposite on the 30th July 1846, when the observations were made. The mark is a cross with the letter V on a small granite block solidly placed on the left of the pathway. The block on the ice is 760 feet distant from the west shore.

By reference to my Eighth Letter, published in this Journal, and to the fuller details in the London Philosophical Transactions for 1846, on the Plasticity of Glacier Ice, it will be recollected that I made observations at a station marked Q between the Angle and Trelaporte. This station was 300 feet from the west moraine. The following observations were made last summer in the same part of the glacier, at a distance of 553 feet from the moraine:—

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* See Fifth Letter on Glaciers.

	Daily Motion. Inches.
July 23—July 30, . . .	21·5
July 30—Aug. 3, . . .	20·7
Aug. 3—Aug. 14, . . .	21·1

A very fine block of granite, marked R in oil paint on three sides, about the same distance as Q from the west moraine (300 feet), had its position fixed on the 26th August 1844, and was found this year to have moved in 693 days, to 20th July 1846, 1108 feet, or at the rate of no less than 19·2 inches daily, or 533·6 feet annually, a remarkably rapid motion, but which was carefully verified by two concordant measures. The rate of motion of this block (engaged as it was in the great crevasses near the Angle), from the 23d to the 30th July 1846, was 11 feet 4·5 inches, or 20·7 inches daily.

B. *At points not before observed.*

I had long wished to ascertain the motion of the ice, which issues from the very remarkable basin-shaped glacier of Talefre. That an oval circus or amphitheatre, whose length may be roughly stated at 4000 yards, its breadth at 2000, entirely filled with snow and ice (excepting the rocky island of the Jardin), should disgorge itself by an icy stream through a chasm less than 700 yards wide in its broadest part, appears to me, as I have elsewhere stated at large,* a plain demonstration of the viscous theory of glaciers; the outlet in this case acting precisely as a stream does to a lake of indefinite extent, as a mere overflow, or *trop-plein*, a fact only consistent with the quasi-fluid motion of the mass so discharged.

The great distance of the Talefre from habitable spots, renders it inconvenient for prolonged experiments, but having last summer twice spent the greater part of a day in its neighbourhood, I have been enabled to determine with exactness its rate of motion.

The part which I selected for experiment, will be understood from the little portion of a map contained in Plate II., fig. 2, on the same scale as my large map of the Mer de Glace. The station marked W, near the foot of the Aiguille du Moine, is a considerable block of granite, a few yards to the left of the usual pathway ascending to the Jardin; and my point of observation was permanently marked (as usual) by a cross cut in the top of the stone, and painted red with oil colour, and the letter W by its side. It is not many minutes walk short of the spot where travellers bound for the Jardin, usually enter on the glacier of Talefre. Hence it will, I hope, be easily recovered. It is about 8700 feet above the sea. From it my mark on the Tacul (station B of my map) could be distinctly seen with the telescope; and a transverse line across the glacier of Talefre, in which marks were placed on the 24th July 1846, made an angle, with the direction of B, equal to 98° 30′. The position of this line of stations is shewn distinctly in the small map. It will be seen that it was near the spot where the contraction of the gorge and the icy stream was greatest, in fact, in the throat of the funnel. The inclination or slope of the glacier in the direction of its motion was inconsiderable, especially near the centre and the southern side; and the transverse crevasses, which are here just beginning to be numerous, are evidently owing to the extended influence of the steep ice fall, which is not many hundred feet in advance of the transverse line selected. The glacier of Talefre, higher up, is remarkably compact and united; the crevasses few and inconsiderable, particularly on the northern side. The crevasses, as they present themselves, are convex towards the origin or basin of the glacier, and are here, as in other cases, perpendicular to the veined structure so far as developed, and which was quite correctly laid down in my map of 1842; the structural bands converging towards the outlet like filaments of water towards the *contracted vein* of a spout. The curvature of the crevasses (seen *in plano*) appeared to me to have a point of contrary flexure, as shewn in the drawing, dividing them into two loops.

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* London Philosophical Magazine, May 1845, page 415. Compare the map of the Mer de Glace in *Travels in the Alps of Savoy*. The outlet of the glacier is seen in Plate II., fig. 2, accompanying the present paper.

Three stations were selected, as shewn in the plan. The central one, marked (2.), was placed on the moraine descending from the Jardin. No. (1.) was nearly midway between it and the northern shore at station W. Here there is a hollow in the surface of the glacier, which was thickly covered with snow in the end of July. At (2.) and (3.) the ice was quite bare and more level, but at the same time more crevassed,—the crevasses being remarkably well defined, narrow, rather deep, and rectilinear, or slightly curved, not uneven. No. (3.) was 1068 feet beyond (2.) No. (1.) was 533 feet northwards from No. (2.), and was 687 feet distant from the northern shore. These distances were ascertained partly by direct measurement, partly trigonometrically. These three points were fixed in the transverse visual line from W. on the 24th July, by forming three perfectly round and vertical holes, no less than 5 feet deep, by means of an iron *jumper*.

Being examined on the 31st July, after a lapse of six days and seventeen hours, the motions were found to have been—

	No. (1.)		No. (2.)		No. (3.)	
	Ft.	In.	Ft.	In.	Ft.	In.
July 24, 5 P.M. to July 31, 10 A.M.,	7	2·5	8	9	7	2·5
Or daily in INCHES, . .		12·9		15·7		12·9

By a remarkable coincidence, the 1st and 3d stations had been so symmetrically chosen, as to have precisely the same velocity. The middle point, as usual, moved fastest.

This velocity will be considered to be large when we recollect the great height of the glacier above the sea, and the small inclination of its surface at this place. On the other hand, it is a natural consequence of the theory which regards this narrow outlet as the overflow of a vast reservoir, in which snow has peculiar facility in accumulating.

I shall now mention a truly remarkable circumstance connected with the velocity of the discharge of the icy stream which empties the basin of Talefre. I was apprised by David Couttet, on the 23d July last, on his return from looking for crystals on the moraines of the Glacier of Talefre, that he had found opposite to *Les Egralets* (see the map), or where the ice of the Talefre joins the Glacier de Lechaud, some fragments of a knapsack lying on the ice, which he at once recognised to be the same as had been lost by a guide some years before, who fell into a crevasse, where he had nearly perished, in conducting a traveller to the Jardin. Couttet rightly believed that the determination of the motion of the ice in the interval, since the loss of the knapsack, would be a matter of interest to me.

At the same time the seeming improbability of recovering so destructible a material as a bag or knapsack, made of cloth, after remaining for ten years in the bowels of a glacier, was so great as to make me resolve to investigate the matter thoroughly whilst on the spot.

The next day I went to the Glacier of Talefre, accompanied by Couttet, Balmat, and another, and Couttet led us straight to the spot marked in the small map (Plate II., fig. 2), with the words "knapsack found," a little higher up than where the usual path to the Jardin by the ascent of "Les Egralets," leads from the ice to the ascent of the rock on the left. Just where the ice of the Talefre incorporates itself with that of Lechaud, and at the distance of 158 feet from the face of rock which bounds the glacier, we found several fragments of strong blue and white cotton stuff (now shewn to the Society) very much worn by friction, but by no means rotten, and not discoloured, with portions of very strong straps and loops partly attached to it of figured tape, which had formed the attachments and shoulder straps (*bretelles*) of the knapsack. Beside them were pieces (or a piece) of bottle glass. Balmat immediately confirmed Couttet's recollection of them as parts of the identical bag lost by Jullien Dévouassou ten years before.

Now, to explain how Couttet and Balmat were in a position to speak so positively to the identity of the fragments, I must observe, that Couttet was then, and has been ever since, lessee of the pavilion at Montanvert, and that the knapsack in question *was his property*, and was left at the Montanvert, for the express purpose of carrying provisions for travellers, who then, much more rarely than at present, visited the Jardin. Auguste Balmat, my guide, was at that time servant to David Couttet, and kept the pavilion under him, and had very often carried on his back this very knapsack. The figured stripe of green and purple on the shoulder straps was very marked, and could not easily be mistaken. The testimony of these two men was therefore perfectly authentic, but I verified it by questioning the very Jullien Michel Dévouassou himself, who is still a guide at Chamouni, and who, on seeing the fragments, offered to verify them upon oath. The accident occurred thus:—On the 29th July 1836 (or ten years all but five days from my recovering the fragments), Dévouassou accompanied a stranger to the Jardin, taking, as usual, the knapsack from the Montanvert, with a supply of bread, cheese, and wine. They arrived without accident at the top of the Couvercle, between the Aiguille du Moine and the Glacier du Talefre; and when at the point marked in the accompanying sketch with the words "Knapsack lost," the guide, to shorten the way, attempted to take an oblique course to the Jardin, instead of following the usual track on firm ground round the foot of the Aiguille du Moine, and then turning sharply to the right, so as to make the passage of the glacier as short as possible. The ice on which he ventured was partly covered with snow, as is almost always the case there in July, and near the edge it was also full of concealed fissures. Into one of these the guide suddenly dropped, leaving the astonished traveller alone in this wilderness of rocks and ice. After vainly calling to his guide and obtaining no answer, he left the place in despair, and returned to the Montanvert by the way he had come. Dévouassou, however, having reached the bottom of the crevasse but little hurt, managed, by the aid of his pocket-knife, to cut steps in the walls of ice, and, finally, with great exertion and suffering, to raise himself to the surface, and make his escape, leaving behind him his knapsack, of which, of course, he had first disembarrassed himself. Astonishing fact! that the yet undecayed vestiges, together with a part of even the very bottle which formed his burden, should be brought to light on the surface of the ice after ten years friction and onward movement! I took pains to measure angles with my theodolite at each point, which enabled me to project them with tolerable accuracy on the map, as shewn in the Plate; and I ascertained by the barometer the difference of level of the two points, and also their elevation above the sea.

	Height above the Sea. English Feet.
Knapsack lost, Glacier du Talefre, 29th July 1836,	8657
Knapsack found, Glacier du Lechaud, 24th July 1846,	7512
Difference of Level, .	1145
Horizontal distance moved over, ...	4300 feet.
Declivity, . . .	14° 55′
Annual motion, . .	430 feet.

This motion harmonises sufficiently well with the numbers found in page 142, if we allow for the acceleration manifestly due to the rapid declivity which commences immediately below station W.

This interesting discovery forms a curious *pendant* to the history of De Saussure's ladder, believed to have been found in fragments opposite Trelaporte, as fully detailed in my Travels, page 86; but this new case is much better ascertained in all its particulars.

I may here mention, as the subject suggests it, another proof of the extraordinarily conservative power of the ice even upon the most seemingly destructible bodies. On the 20th July 1846, I found on the surface of the ice at station Q (opposite the Glacier of Charmoz), all within a few feet of each other, four of the small wooden pins which I had left there in making the experiments in August 1844, described in my Eighth Letter, a quantity of the thin string, and pieces of the birch broom then used, and even a quantity of the loose straw which I used to wrap round my shoes in cold weather. These were neither decomposed nor blown away by the winds, nor had they fallen into any of the numerous crevasses, but were lying on the *surface* of the glacier (a very different surface of course from that on which they had really been left), as if they had been in use at most but a few weeks before.

Glacier du Nant Blanc.—This is a small glacier descending from the foot of the Aiguille de Dru, exactly opposite the Montanvert. I visited it for the first time in 1846. Its form and position will be understood from a reference to the general Map of the Mer de Glace. It is shaped somewhat like a tongue, and is steep, without being much crevassed or very uneven, at least in its middle region, where it is about 1000 feet broad, where cattle are every year compelled to traverse it; yet even there it has an inclination of 18° or 20° near its centre. This glacier terminates at a considerable height above the Mer de Glace, and gives rise to the Nant Blanc, a torrent which gives its name to the glacier, and whose volume and appearance is subject to remarkable changes, depending upon the state of the weather. It is supported on a vast pile of blocks which it has brought down, and which forms its moraine; and whilst on the left bank the ice overhangs the moraine, on the right, the latter being elevated in a mound, forms a barrier to the former. This glacier has a very small and crevassed *névé*, taking its origin amongst the rugged rocks between the Aiguilles of Dru and Bochard. The veined structure of the ice of the glacier is perfectly normal, exhibiting the parabolic loops with the frontal dip inwards. Its general form resembles that of the Glacier des Bossons.

I stationed my theodolite on the northern moraine, and fixed a station towards the centre of the glacier, or rather on the axis of the structural bands, which usually intimates the point of swiftest motion. The inclination of the glacier was here, as already mentioned, 18° or 20°. Notwithstanding this great declivity, the mean motion from August 4 to August 15, was only 9·9 inches daily.

Glacier du Miage.—In order to complete here the observations on the velocity of motion of different glaciers, I shall include those made also on the south side of Mont Blanc. I had intended to have made a considerable number of experiments on the remarkable Glacier du Miage, fully described in my Travels, Chap. X., but I found it less suitable than I expected. I renewed, however, my survey of all the middle region of the glacier; and I ascertained the motion of its central part, between the two medial moraines at its issue into the valley from between the colossal summits of the Aiguilles Rouges and Mont Broglia; in fact, at a point near the letter *r* of the word "Glacier" on the eye-sketch of a ground plan of the glacier in my published volume. From the 7th to the 10th August, I found the *daily* motion to be 9·9 inches; or the very same as that just given for the Glacier de Nant Blanc, than which, two cannot be more dissimilar. But the smallness of the motion in the latter case, is due to the great height and very small *névé* or reservoir; in the former, to the exceedingly slight declivity and enormous frontal resistance to the exit of the glacier into the Allée Blanche. already encumbered with its ponderous mass and debris.

Whilst speaking of the Glacier de Miage, I may add, that I found on the most westerly of the medial moraines, dark blue lias-schist, which undoubtedly comes from the very axis of this granitic chain, where it has not hitherto been suspected to exist. Also, that a repeated examination of the curious "ancient moraines," forming semicircular embankments towards the Lake of Combal, described in my Travels, p. 194, and figured in the eye-sketch, has convinced me that these cannot be of so old a date as I then believed; but are due to a temporary outbreak of the glacier at some not very remote period, when the accumulation of pressure of the ice has been such, that an *overflow* took place laterally, which deposited these moraines in succession as it retired, and the glacier retreated to its present limit. I found an exactly analogous case in the Glacier des Bois, at Chamouni; which, at some period not on record, has thrown out an arm between the Chapeau and the Côte du Piget.

Having fully described the measures of the Glacier of La Brenva in my last letter, I shall not here return to them.

Thus I have been able to add the velocity of four additional glaciers to the small number previously ascertained.

(2.) ON THE CONVERSION OF THE NÉVÉ INTO ICE.

I shall now add a few observations tending to throw light on two of the most obscure glacial phenomena, first, the conversion of the snow of the Névé into pure ice, and, secondly, on the apparent ejection of stones from the surface of the glacier.

On the first point, I made some interesting observations on the higher part of the Glacier du Géant, where the still snowy ice, marked with the horizontal annual strata, is shoved violently down the steep, which occasions the scene of desolate confusion between the Aiguille Noire and the rock called Le Petit Rognon. The structure of the interior of the embryo glacier is here perfectly disclosed by the prodigious vertical rents which make the scene a true giant's staircase; the ice-falls succeeding one another at regulated intervals, which appear to correspond to the renewal of each summer's activity in these realms of almost perpetual frost, when a swifter motion occasions a more rapid and wholesale projection of the mass over the steep, thus forming curvilinear terraces like vast stairs, which appear afterwards, by consolidation, to form the remarkable protuberant wrinkles on the surface of the Glacier du Géant described in my Fifth Letter.* But the point which at present concerns us is this, that, according to the best observations which I could make, the stratified appearance of the *Névé* disappeared at a depth inconsiderable compared to the vast vertical sections there exposed, and the interior of the mass was *granular, and without structure or bands of any kind.*

I drew the very same conclusion from an attentive survey of the Glacier de Talefre, which is peculiarly calculated to throw light upon this question, and I shall state the result of my observations there nearly in the words in which I recorded them immediately after they were made. A little higher up than the usual passage of the glacier to the Jardin, or near the upper limit of the sketch, Plate II., fig. 2, the annual layers of the *Névé* are seen. But these wholly disappear farther down; the great body of the ice in the hollow of the basin exhibits little sign of structure; only near the *lateral and medial moraines* the structure is icy and vertically veined, elsewhere it is *decidedly snowy*, with hardly any trace of bands either vertical or horizontal. This is true even as far down as the line of stations W in the figure. The general disposition of the structural lines, where they exist (which, it is to be recollected, are almost vertical), is seen in the same figure perpendicular to the crevasses. From these facts I conclude, *first*, That the vertical structure is too close to the original strata of the *Névé* to allow of the supposition that these have all of a sudden turned up vertically in some parts of the glacier, and disappeared in the remainder. *2dly*, That where the vertical bands are not developed in the higher glacier, the structure remains snowy and undefined. *3dly*, That *the conversion into ice is simultaneous, and in this case identical with the formation of the blue bands*. *4thly*, That these bands are formed where the pressure is most intense, and where the differential motion of the parts is a maximum, that is, near the walls of the glacier; but being once formed, it still continues, at least for a time, to be observed under the medial moraine, and this may even be traced throughout the ice-fall of the Talefre.

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* It is not unimportant for travellers to be aware that in seasons like 1846, of unusual warmth and activity amongst the glaciers, the dislocation and precipitous subsidence of the tabular masses of the Névé is occasionally so complete as absolutely to debar a passage, at least without the help of a ladder. This was the case when I ascended the Glacier du Géant on the 14th August last, a snow bridge by which some travellers had effected a passage a fortnight before having wasted away. Had travellers at that time crossed the Col du Géant from Courmayeur to Chamouni, they might have found their descent, if not impracticable, at least most perilous, and to return from such a distance would have been an almost equally distressing alternative. On this account it is most advisable to make this passage from the side of Chamouni.

I am satisfied then (and it is only after long doubt that I venture this confident expression), that the conversion of snow into ice is due to the effects of pressure upon the loose and porous structure of the former; that the very first effect is to annihilate the annual strata of the *Névé*, and that the most rapid glacification is effected by the kneading or working of the parts upon one another, by the differential motions which the semi-fluid law of glacier progression occasions, and which also necessarily takes place under intense pressure.

The belief which I formerly (in common, probably, with most other persons) entertained, that snow could not pass into pellucid ice without being first melted and then frozen, was part of the chemical prejudice that molecular actions cannot take place except in the liquid state, a prejudice now disappearing, as the subjoined note, on the very competent authority of M. Gay Lussac, shews.[*] The crystalline forces act on the snowy granules when brought into close contact by pressure, and the imprisoned air is then distributed in the direction of the *lines of tearing*, in the form of layers of regular globules, just as in the case of the banded lavas which have been so well described by Mr Darwin.[†] Bishop Rendu, whom I had the pleasure of visiting at Annecy, remarked a familiar circumstance which illustrates the same thing. We often see, in the coldest weather, that opaque snow is converted into translucent ice by the sliding of boys on its surface; friction and pressure alone, without the slightest thaw, effect the change, which must take place still more readily in the glacier, where the mass is, during a great part of the year, kept on the very border of thawing, by the ice-cold water which infiltrates it. In this condition, molecular attachment amongst the granules must be comparatively easy, and the opacity disappears in proportion as optical contact is attained. Most evidently, also, the icy structure is first induced near the sides of the glacier where the pressure and working of the interior of the ice, accompanied with intense friction, comes into play, and the multitudinous incipient fissures occasioned by the intense strain, are reunited by the simple effects of time and cohesion.[‡]

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[*] "Il n'est plus permis aujourdhui d'avoir une foi aveugle au principe si banalement répété des anciens Chimistes, *corpora non agunt nisi soluta*. Il est certain, au contraire, que tous les corps, solides, liquides, et aériformes, agissent les uns sur les autres, mais que, des trois états des corps, l'état solide est le moins favorable à l'exercice de l'affinité."—*Annales de Chimie et de Physique*, Juin 1846, p. 231.

[†] On Volcanic Islands, and in Philosophical Magazine, April 1845.

[‡] A very remarkable peculiarity is observed in some of the glaciers of Switzerland, which distinguishes them from the more rapid and precipitous ones of Savoy. The glaciers of the Aar, Rhone, and Great Aletch, exhibit a degree of *crystalline structure* which I have nowhere else observed; broad, laminated, crystalline plates (not unlike, in general size and polish, to those of hypersthene in hypersthene rock), shew a development of crystallizing force which must evidently be the effect of long time, and probably of comparatively slow motion. The reflection of the sun or moon-beam from these plates gives to the glaciers I have mentioned a dazzling effect, which I have not observed on the glaciers of the Pennine Chain.

We are, therefore, relieved from the difficulty of accounting for the cold which would be necessary to freeze the infiltrated water which was at one time believed necessary to explain the conversion of the *névé* into proper ice. This would be liable to most of the objections urged against the Dilatation Theory.

(3.) ON THE FIRST APPEARANCE OF STONES (ERRATICS) ON THE SURFACE OF GLACIERS.

I shall conclude this letter with a few very partial observations upon what I must consider as one of the phenomena of glaciers, still obscure as to its explanation, although most familiar as a fact,—I mean their supposed tendency to reject impurities, and the undoubted fact that stones are always found near or upon the surface of the ice. It is strange that it should not have occurred to every one who sought to explain the appearance of stones on the surface by the *ablation* of the ice, that in order to arrive there at all, the blocks must previously have been imbedded in the virgin ice, where popular belief, and, generally speaking, more accurate observations also, give them no place. Yet, in the thousands of crevasses which an active observer passes and examines in a week, how few cases of imbedded stones in these vast vertical sections are ever visible ? I might almost ask whether they are ever seen, except in the neighbourhood of the sides of a glacier, *i. e.*, under the lateral moraines ; and even there how rare !

The object of the present memorandum is, on the one hand, to direct attention anew to the apparently perpetual paradox which the glaciers present in this respect, and which, I am persuaded, offers something yet for careful observation, and, on the other, to give what seems to me incontrovertible evidence, that what I have so seldom seen myself must yet exist, and that stones are actually extruded, although in a peculiar manner, from pure ice, or at least *exposed* by the ablation of the surface.

In my Travels, p. 241, referring to the excessively gradual development of the moraine of La Noire on the Mer de Glace of Chamoni, I stated my belief, that this moraine was buried in a fold between two glaciers, one of which had overflowed the other, and that, as the upper glacier decayed away, the rocky fragments were strewed on the surface. A fresh examination of the same localities leaves me in the same want of direct proof of this fact, but the difficulty of explaining it otherwise, makes me suppose my former view correct.

The extruded stones on the Glacier du Nant Blanc, near Chamouni (alluded to in the first part of this letter), present a very remarkable appearance, imperfectly shewn in Plate II., fig. 6. The right bank of this glacier (that which appears on the left of the figure as seen from a distance) is at first bounded by rocky summits, but in the lower part of its course by a mound-like moraine of the usual form. The surface-blocks can only be derived from the precipices near the origin. Yet they do not even appear on the surface opposite to the rocks, but only opposite to the moraine; and they increase in number and quantity towards the lower end of the glacier, where they almost blacken the surface of the right side, the left side remaining almost clean. It is difficult to believe that this accumulatiom is not due to the gradual denudation of the blocks by the melting of the ice in which they have been, in some way or other, imbedded, but it is scarcely less difficult to admit that, having fallen from the rocks above the Névé, they should have remained unperceived in the ice during all the intermediate space.

To take another example. The glacier of the Rhone is distinguished by the extraordinary purity of its surface, and the consequent absence of lateral moraines. But this general freedom from stones on the surface is subject to one exception, which is remarkable :—*Stones begin to appear at the surface on the terminal slope* at a *considerable height.* How came they there? Not a stone the size of the fist can be seen on the surface farther up; and, in examining a number of the crevasses, I could not see any engorged in the ice. The explanation seems to be that the stones are actually introduced into the ice by friction at the bottom of the glacier, and forced upwards by the action of the *frontal resistance* which produces the *frontal dip* of the veined structure, and they are finally dispersed on the surface by the melting of the ice. What is here supposed to occur is illustrated by an ideal section of the glacier of the Rhone, Plate II., fig. 7, where the curves of ejection are identical with those of *forced separation*, causing the frontal dip of the veined structure ; and this view is confirmed by what I have often observed, particularly on the Glacier of Bossons, that the veined structure in contact with the lateral moraines becomes soiled, and that dirt and stones may be traced along the course of the structural bands from the moraine to a considerable depth in the ice. The action there is in the horizontal plane, what we here suppose to take place in the vertical, and which the now established retardation of the lower strata permits us to assume as exactly a similar action. I have no doubt that a similar explanation applies to the glacier of the Nant Blanc, and to other glaciers.*

Before closing this already too long letter, I wish to record an observation already made by me in 1844, but which I hesitated to publish because the sketch representing it was made from memory, and not upon the spot; but I have now verified it both in the same and another locality. It is represented in Plate II., fig. 5, where *b d* is part of the wall of the glacier, which is about to turn at a considerable angle with its former direction (it is at the well-known part of the Mer de Glace named *L'Angle*). I knew, from long experience, that the ice here presents, year after year, compact ridges, such as *a b*, *c d*, parallel to one another, and separated by a mass of crevasses which it is, generally speaking, impossible to cross; and any one who attempts to traverse the more crevassed portions of the glacier without attending to this peculiarity, will infallibly lose his way. But, as the drawing explains, the direction of these ridges by no means corresponds with that of the individual crevasses, whose grouping subdivides the glacier as I have now described. The crevasses may be nearly transverse to the glacier, whilst the *systems* of crevasses form an angle of perhaps 30° with the transverse line. The veined structure again cuts the crevasses at right angles, so that these may be regarded as three orders of discontinuity, or tearing surfaces, which occur in *systems*, that is, regularly repeated at nearly uniform intervals over great portions of the glacier surface. I have this year succeeded, for the first time, in laying down on a map, an approximation to the various and complex systems of crevasses which traverse the Mer de Glace, and I have found a repetition of this phenomenon of a series of discontinuous but parallel fissures ranged along a line or axis oblique to their direction, to recur at several points where the strain is very violent. Let it be remarked, too, that where the violence of the pressure opens a system of such fissures to relieve it, the bands, or system of surfaces of molecular discontinuity, disappear, or are less well developed. I remain, my dear Sir, yours sincerely,

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* It will be seen that this explanation will give an elevatory force to the ice containing blocks similar to that which De Charpentier (*Essai sur les Glaciers*, § 25) ascribed to the expansion of the frozen water. It will also be seen that it renders a perfect account of the "veins of the debris of rocks" in glaciers, particularly near their lower extremities, which that ingenious author has attempted to account for (unsatisfactorily, I think) by the transporting action of streams of water.—(*Ibid.*, § 27).

JAMES D. FORBES.

12TH DECEMBER 1846.

On Polished and Striated Rocks, lately discovered on Arthur Seat, and other places near Edinburgh. By DAVID MILNE, Esq., one of the Secretaries of the Royal Society of Edinburgh.*

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For the last thirty years, much interest has prevailed among geologists, connected with those appearances on the surface of hard rocks, which clearly indicate that they have been ground down or polished, and sometimes furrowed or scratched, by a foreign agent. So long ago as 1812,† the late Sir James Hall called attention to the *dressings* (as he termed them) of the rocks on Corstorphine Hill, consisting of smoothed surfaces, marked also by ruts and scratches, all more or less parallel to each other.

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* Read before the Royal Society of Edinburgh, on 20th April 1846.

† Transactions of the Royal Society of Edinburgh, vol. vii.

These observations of Sir James Hall have lost none of their interest by the lapse of time. On the contrary, their importance with reference to geological speculations, has vastly increased,—*first*, by the discovery that the same phenomenon occurs in multitudes of other places throughout this country, and in others; and, *secondly*, by the vain, at all events diversified, attempts to assign an adequate and probable cause for the phenomenon.

Sir James Hall suggested the notion of a tremendous wave which swept over the country; and in this suggestion most people were contented to rest, until the theories of Glaciers and Icebergs came into fashion; and, supported as they have been by philosophers of great talent and reputation, the question as to the origin of the polishing and scratching which exists on the surface of rocks, now forms one of the most difficult and controverted in modern geology.

I do not wish, however, to be understood as stating, that in all the cases where the phenomenon occurs, geologists are completely baffled as to a probable explanation. In Switzerland, for example, it is now generally allowed, that the smoothed and striated appearances presented by the rocks situated in its valleys, and even considerably beyond them, are to be explained by the action of glaciers moving over them. An attempt has been made to extend this explanation to Scotland; but, with the exception of Professor Forbes's Memoir on the Cuchullin Hills of Skye, I must say, that none of these attempts ever brought conviction to my mind. The proposal of those who first searched the country in quest of moraines, was to cover the whole of Scotland, lowlands as well as Highland glens, with sheets of ice, which, moving forward even on plains the most level and the nearest to the sea, ground down the rocks, and occasionally rutted them with the loose blocks encased in the body of the glacier; and, so far as I can learn, this theory is not yet abandoned. Professor Forbes, on the other hand, has limited his glacier views, in so far as yet made known, to the explanation of polishing and scratching in valleys and the mouths of valleys, which have not only a considerable slope, but are connected with ranges of hills having a considerable height.

In such cases as these, there is not much difficulty in applying the glacial hypothesis. But when the phenomenon occurs where there are no valleys at all, and where, if a glacier existed, it must have moved in a way entirely different from what the scratches indicate, then arises the problem, as to the solution of which geologists are by no means agreed.

I have been led into these preliminary remarks, to shew the great geological value of the discovery recently made on the west side of Arthur Seat. In forming a part of that magnificient carriage-drive which is to encircle the hill, the work-people came upon those rocks, a description of which will occupy the chief part of this paper. The value of the discovery is great for two reasons: In the *first* place, the spot may be visited by thousands of persons, without more trouble than is required for a pleasant half-hour's walk; whereas most of the places where similar phenomena have been previously observed are remote and inaccessible, in which case credit must be reposed on the accurate perception and description of a single or few observers. In the *second* place, the nature of the locality is such, as to preclude the applicability of some of the hypotheses which have in other cases been admissible for explanation.

For these reasons, it occurred to me as desirable to make known and record the appearances presented at Sampson's Ribs, bearing as they do so materially on those curious speculations regarding revolutions on the earth's surface, which are supposed to have occurred at a comparatively recent period. I expressed a wish that others more competent, and with better opportunities of observation, would undertake this duty, but without success.

I. The spot where the rocks in question occur, is in a small hollow between the main body of Arthur Seat, and that outlying portion of it which has received the name of Sampson's Ribs, from its beautiful basaltic columns. These columns form part of a hummock or knoll, which is separated from Arthur Seat by a hollow or gully opening out freely at each end,—towards Duddingston Loch at its SE. end, towards Edinburgh at its NW. end.

Through this hollow or gully the new road passes; and the level of the road is 27 feet below the top of Sampson's Ribs, from which it is distant probably about 100 yards. Rock has been found at different parts on each side of the road; and there the rocks on each side are converging downwards towards each other, as if to meet under or below the road, and where they probably do meet at from 10 to 20 feet below its level. They form here, therefore, a *trough*, one side of which is the base of Arthur Seat, and the other, the knoll of Sampson's Ribs.

The width of this trough or gully, *i. e.*, the distance between the rocks on each side of it, is (at the level of the road) about 30 feet; and (about 6 feet higher up) about 38 or 40 feet. At the level of the top of Sampson's Ribs the width, or, in other words, the distance to Arthur Seat is about 140 feet.

The gorge or gully now described runs about NW. and SE. by compass. The top of Arthur Seat, if it could be seen (for the sides of the hill, which rise up almost vertically from this trough, conceal the view of it), would bear about east by compass. The top of Sampson's Ribs is about 420 feet below the top of Arthur Seat, and about 200 feet above the average surface of the low level country which extends from the foot of Sampson's Ribs southward and eastward for some miles. The gully itself is about 380 feet above the sea.

It is farther proper to observe, that the gully in question, which in its extreme length does not exceed 120 yards in length, opens freely at both ends. Indeed, the narrowest part of it, is not at either end, though it is not far from the north end. But in passing from that point where the sides of the trough approach nearest, it gradually opens out or increases in horizontal width both ways. Farther, at the north end, the surface of the ground suddenly slopes away, and sinks down from 80 to 100 feet. At its south end, the ground slopes down with still greater rapidity, and presents almost a precipitous face above the level plain, which is about 200 feet below.

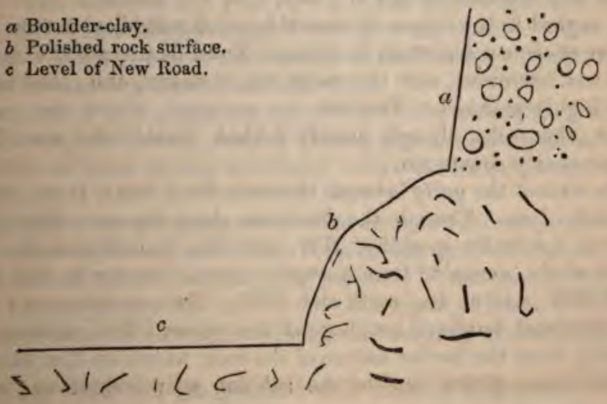
The greatest extent of polished surface is on the *east* side of the new road, and forms part of the *west* base of Arthur Seat. There the rocks, for about 80 yards continuously, have evidently been rubbed, and in some places very highly polished.

On the *west* side of the new road, there has lately been exposed a surface of rock about 20 feet in length, also very highly polished.

On this west side, the polished rock extends upwards for about 4½ feet above the road; it is unknown how far it extends below; and it would be very interesting to discover. The face of the rock here is exceedingly steep; in the part close to the road (and therefore nearest to the bottom of the trough) forming an angle with the horizon of 65°,—and about 5 feet above, 47°.

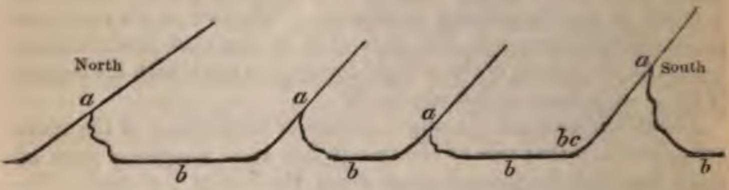
The debris conceals the upper surface or termination of the rock; but apparently that side of the trough has been altogether from 15 to 20 feet high for some distance along the line of the road. The rock was at most places, on the east side where exposed, almost vertical, and at the narrowest part of the gully towards the north, it was almost opposite the polished rocks on the west side of the road. Towards the eastward, where apparently the rock is more friable, the polished surface does not rise so high above the road, and the polished surface was less steep, and in some places almost horizontal. It there presented the following outline.

Fig. 1.



It is material to observe, that whilst all along the east side of the road, there was a general surface of smoothed and polished rock for about 80 yards, spots were here and there found, wanting that appearance. Those parts of the rock had their rough faces uniformly towards the south, or more generally south-west. They were apparently on lines of natural fissures in the rock, which fissures (termed *backs* by quarrymen) run here WNW. by compass. Towards the eastward, the porphyry is much intersected by these fissures, and is in its own substance very friable; whilst, at the narrowest part of the trough, the porphyry is exceedingly compact. In many of these fissures I observed that portions of the rock had been removed, the removed portions having invariably been taken from the south sides of the fissures, thus:

Fig. 2.



The letters *a* represent the fissures, from the south sides of which portions of rock had been removed; the letters *b* represent the general line of smoothing along the east side of the gully. The sides of these fissures facing the south had evidently not been rubbed, but preserved their original roughness in most cases. Indeed, I found only one exception, and that was, where there was an unusually larger portion of the rock removed from the south side of the fissures in the way represented at *b c*.

In regard to the degree of smoothing and polishing, it was much greater at some places than at others. Towards the west, where the gully was narrowest, and the rocks were hardest, the rocks are the most highly polished. Towards the eastward, where the rock is friable, the rocks, though greatly rubbed, rutted, and worn down, are not nearly so smooth.

The axis of the gully between these smoothed rocks, is not exactly a straight line. Though the direction along the east side of the gully is, generally speaking, NW. and SE. by compass, the line, on the whole, seems to form a gentle curve, running at the north end WNW., and at the south end SSE. Even in this curve there are occasional bulgings out beyond the general line, arising, apparently, from the harder nature of the rock, which, on that account, seems to have yielded less to the rubbing agent, whatever it was;

and at these prominences the faces looking north are much smoother than those looking south.

The direction of the polished wall on the west side of the road is apparently not parallel to that of the rock immediately opposite to it. The rock exactly opposite is not seen, being probably covered by debris; but if it were uncovered, it would probably be found running NW. by W., judging from the parts nearest it, whereas the polished rock on the west side runs NNW.

Having now described the polished and smoothed condition of the rocks, I proceed to describe the furrows and scratches on them.

(1.) In the *first* place, they are quite innumerable. They are to be seen on almost every part of the rocks, which have been polished or rubbed.

(2.) They, of course, vary in length, from a few inches to several feet. One of them I traced continuously for six feet.

(3.) The direction of them coincides, generally speaking, with that of the gully. When seen on the steep or vertical wall-faces of the rock, they are, in almost all cases, found to be horizontal. But at the places where the polished rock-surface projects beyond the general line, the furrows and scratchings are not horizontal; some of them slope to the horizon as much as 4°, or even 5°.

In all the cases of this kind, the sloping lines are on the faces of the rocks which project beyond the average line of the side into the gully. Their dip is to the north.

There is one rock-face, however, on which the scratches dip, though, apparently, the rock does not project beyond the general line of the side. This is on the west side of the road. Close to this wall-face there exists a large basaltic boulder, which, to a considerable extent, has choked up the gully at this point, and, of course, rendered the passage of any body through it more difficult. On the NE. side of the boulder itself, there are some well-marked ruts and scratches.

(4.) The scratches and ruts vary in depth and width, as much as in length. Generally speaking, the longest are also the widest and deepest. The one before referred to as six feet long, is about one inch wide, and one-third of an inch deep.

It appeared to me, that the longest and deepest scratches are in the following places, viz., *first*, on the horizontal rock-surfaces; and, *secondly*, on those sloping wall-faces, where the scratches dip to the north.

Generally, the scratches seem to be the most numerous at a low level; and on those parts of the rocks facing the SW., which were only a little, if at all, polished, there was only a faint trace of scratching. In one place, viz., at *b c* on the foregoing figure, where the SW. face was rubbed and smoothed, there were two or three scratches visible.

Having described the polished and striated appearance of the rocks at this place, I proceed to mention some collateral points, which appear to deserve attention.

1. The *superficial deposits* over these rocks are of two kinds.

The one lying immediately on them, is a clay of a light brown colour, very stiff and tenacious. In order to be removed, it must be worked with the pick. There are many blocks of stone, of various sizes, interspersed through the clay, some of them rounded, and others angular, the nature of which will be immediately described.

This stiff clay is not stratified, and it resembles, in many respects, the well-known Till or Boulder clay which prevails over the rocks in the Lothians. The only difference is, that it wants the bluish-black colour which almost everywhere characterises the boulder clay, and that it is not quite so tenacious.

At Sampson's Ribs, this deposit is in immediate contact with the polished and striated wall faces of the trough or gully, which, in fact, had been filled by it; and it is to this impervious covering of the rocks that we seem to be indebted for the very perfect preservation of the striæ. Many of these striæ are exceedingly minute, so that they could have been easily obliterated by weathering; and from this circumstance, as well as from the aspect of the whole surface when the clay covering is removed from it, the idea is strongly suggested, that little or no time could have elapsed between the formation of the striæ and the deposit of this clay bed.

The largest of the boulders imbedded in this clay which I observed, is the one before specially noticed, as having partly choked up or filled the gully at or near its north opening. It was very much rounded, and had an average diameter of from three to four feet. I observed five or six other blocks nearly as large near it, but, generally speaking, the blocks were about half the size of the one now mentioned.

They were not deposited in any regular order, but just as they are usually seen in the well-known Till or Boulder clay of Scotland.

Whilst some of them were rounded, others of them were angular. The smallest in size were generally the most angular.

Multitudes of both classes of boulders were lying in immediate contact with the polished and striated rocks,—not only where the surface of these rocks was horizontal, but where they sloped even at the most considerable angle to the centre of the trough.

As to the nature of these imbedded boulders, they were of two kinds. Some of them, being very compact and much rounded, had evidently been brought from a distance, an inference confirmed by the fact, that some of them were of rocks, none of which occur at this place or in the neighbourhood of it; others, again, being soft and friable, and consisting of rocks the same as those occurring in the neighbourhood. But in both classes, there was no imbedded mass which could be discovered in the clay of a rock precisely the same as that forming the gully itself.

The following is a list of the different kinds of rock composing the boulders.*

(1.) Compact felspar, and felspar porphyry, of red, yellow, and fawn colours. These are pretty numerous, and some are about the size of a boy's head. They are not much rounded, and generally of an angular shape. They are exceedingly hard.

(2.) Coarse greenstone porphyry, almost a sienitic porphyry, in blocks about the same size as the former; they also are angular in shape.

(3.) Blocks of claystone tufa, containing zeolite and other kindred crystals.

(4.) Blocks of greenstone and basalt.

(5.) Limestone, very compact, containing two species of *Entomostraca*, and some bituminous casts of *Ferns* and *Equiseta*. The specimen which I found is of a dark grey colour, with a tinge of lilac. A workman, to whom I shewed it, declared it to be identical with the upper bed of the Burdiehouse limestone, which he had often worked.

(6.) Limestone of a more lilac colour and coarser in the grain, in which no fossils are distinguishable.

(7.) Marine limestone. I found two kinds,—one of a yellow colour, and coarse in the texture, containing a large *Productus*, fully 3 inches in diameter; the other of a dark colour, and very minute texture, full of marine shells. Of this last kind, some of the specimens were slaty, almost approaching to shale; others were compact, resembling the ordinary limestone which is quarried for use.†

(8.) A few masses of coarse red sandstone, containing quartz pebbles, apparently of the old red sandstone formation.

(9.) Rounded pebbles of pure quartz, similar to what occur in the old red sandstone conglomerate.

(10.) Soft yellow sandstone (of the coal-formation), containing cast of a *Lepidodendron*.

(11.) Soft brown marly sandstone (of the coal-formation), containing portions of *Stigmaria*.

(12.) Numerous pieces of soft white coal sandstone.

(13.) Fragments of shale and coal.

(14.) Blocks of greywacke.

The boulders now described were imbedded in the clay which formed the first deposit immediately above and in actual contact with the smoothed rocks in the gorge.

Mr Maclaren, in his Sketch of the Geology of Fife and the Lothians, states that at the bottom of Sampson's Ribs, where the railway tunnel now is, a clay deposit was discovered when the tunnel was formed. The characters given of it correspond with those of the deposit just described as filling the gorge. Mr Maclaren states that it lay immediately on the rocks, that it was of a yellowish colour, that it contained blocks of sandstone which were angular, and rounded blocks of greenstone not belonging to the rock above; lastly, that it was covered with the debris of the hill. In all these respects, the two deposits seem to me to agree.

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* Specimens have been deposited in the Museum of the Royal Society, where also will be found portions of the smoothed and striated surfaces of the rocks.

† I found among the clay several septa of the Encrinite, derived probably from the marine limestone blocks.

It is of some importance to ascertain their identity, because, if this is made out, it would shew that when they were deposited, the surface of the country must have been much the same as at present; at least, that nothing has since occurred to remove the deposit from either of the two places, or to change the levels. Farther, it is more than probable, that the clay and boulders were at both places deposited at the same time and by the same agent, whatever that was.

Immediately above this clay deposit in the gorge, there is a bed of debris, formed apparently by the crumbling of the superjacent rocks. It is from 6 to 10 feet thick. The only circumstance of interest connected with this deposit is, that sea-shells have been found in it, viz., Limpets, Whelks, and the common large Bucky. The two former, I understand, were found by Mr Nichol. The last I found myself. I do not understand that any of these shells occurred in a bed or other apparently natural state; and the general opinion seems to be, that being all edible, they had been carried there. At the same time, it is deserving of remark, that they were found considerably below the present surface of the ground. These shells were found very nearly in contact with human bones, and the bones of the horse, cow, deer, and pig. Close to the spot, there were also found Roman swords and coins.*

In connection with these superficial deposits in the gorge, notice may be taken of two other deposits which occur some hundred yards to the eastward, also on the line of the new road.

One of these deposits is a bed of sand and clay intermixed, full of water-worn stones, from the size of a cocoa-nut and less. At the place where it occurs, a little to the west of Windygowl, the hill-side is very precipitous and rocky. It evidently there occupies, and fills up, a hollow or recess in the hill-side, because at the east, as well as west end of this sandy deposit, the rocks advance beyond it. The sand and clay exhibit traces of horizontal stratification, and the bed extends for about fifty yards along the north side of the new carriage drive. The great antiquity of this deposit is, to a certain extent, shewn by the enormous mass of rocky fragments, sharp and angular, which have fallen upon it from the overhanging cliffs. One of these blocks, about a ton in weight probably, is partly imbedded in it, and the horizontal layers of sand are to be seen encircling the lower half of it. It is important to observe, that this fragment is rounded and smoothed at its west end, but is rough and angular on all the other parts exposed. The gravel and travelled blocks which have been apparently brought and laid down simultaneously with the sand, consist chiefly of greenstones, basalts, and hard sandstones, of a white and grey colour. These are of the same character as the rocks which occur in Salisbury Crags. But there are other boulders in this sandy deposit, which cannot be identified with any rocks in the neighbourhood. These are, rounded blocks of red compact felspar, a red sienitic porphyry, marine limestone full of encrinites, and ball or clay ironstone. The depth of this sandy deposit is unknown, as the new road does not reach to the bottom of it;—what is seen is from 4 to 6 feet thick.

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* At Inveresk some years ago, in my brother's garden, similar remains were found, viz., long tusks of the pig, urn, with a Roman name stamped on the bottom, containing human bones, and a Denarius of Vespasian.

The other deposit of a similar character is immediately above Windygowl. It consists of coarse gravel, indistinctly stratified, and forming a bed about four feet in thickness, which slopes to the SE., at an angle of about 30°. It so happens that the rocks, over which they lie, are red sandstone strata, and that they dip the same way, and at the same angle. It has been accordingly suggested, that this bed of gravel was deposited before these strata were tilted up, and that they acquired their dip by the elevation of the strata. But the more probable explanation is, that the surface on which they were thrown down, had previously a dip to the SE. The gravel consists of rounded pieces of greywacke, porphyry, compact felspar, and sandstone.

2. The next point of a collateral nature deserving attention, is the occurrence of an immense number of coarse greenstone boulders to the east of Wyndygowl. Those spread over the surface of the hill are well described by Mr Maclaren. Many more have lately been excavated, both in the operations for forming the Victoria Road, and in those of the North British Railway Company, near Parsons Green.

I may take the opportunity of here mentioning, that many of the large boulders exposed in the cuttings of the North British Railway present polishing and striæ precisely similar to the appearances at Sampson's Ribs. I lately examined four or five large boulders imbedded in the stiff blue clay before referred to, at Easter Duddingston, the upper surfaces of which were flat and very smooth, and presented strong marks of rubbing. On these surfaces there were scratches, and in some cases ruts, which almost invariably run in directions by compass, varying from NW. to WNW. Two of these blocks were old red sandstone conglomerate, one of them old porphyry, and a third a black basalt; all being rocks which do not occur in the neighbourhood, but most of which exist in Stirlingshire.

At the sea-shore between Joppa and Magdalene Bridge, the boulder clay is well exposed, and numbers of fragments of these old rocks may be seen sticking out from it, partly undermined by the sea. The scratches and ruts on them, run NNW. by compass. Several of these boulders are scratched and furrowed on their west sides,—none of them on their east sides,—a circumstance which shews plainly that the scratching agent had come from the NW. and not from the SE.

This boulder-clay covers the sandstone of Granton Quarry, and contains a number of large boulders scratched and furrowed on the upper surface. The striæ all run east and west by compass. The boulders all belong to rocks in the west country.

I may add that, when in East Lothian lately, I observed that the Railway had cut through this same boulder-clay near Beltonford. Rounded blocks, precisely similar to those above mentioned, are interspersed through it, and also presenting like marks of rubbing and scratching. I took the bearings of the striæ on six or seven boulders, and they were invariably WNW., or more correctly, N. 72° W. One of the boulders was so placed, and so marked, as to prove distinctly that its *lee* side had been to the eastward,—that being the only side free from *striæ*, and other marks of violent abrasion.

A more important discovery was made near Linton, presenting phenomena very similar to those at Sampson's Ribs. The railway (about a quarter of a mile to the west of the station) cuts through a knoll of felspar porphyry, known by the name of Markle Ridge. The distance from the west side of the knoll to the east side along the railway is about 60 yards. Both sides of the knoll, and especially the northwest side, present faces which have been smoothed and striated. On the west side, the extent of smoothed surface is about 50 feet in length, and at one place about 15 feet high. The side of the knoll here gradually bends round towards the north, to meet the opposite or east side of the knoll. Accordingly, the smoothed surface on the west side faces successively (reckoning from the south) NW., then NNW., and N. by W. by compass. The small portion of smoothed rock on the opposite side of the knoll faces SE. by compass. The rock on the north side of the knoll is covered by debris, so that it cannot be seen whether it presents any similar appearances.

These smooth faces, on both sides of the knoll, exhibit ruts and scratches, the most numerous and deeply marked being on the west side. Their direction is ENE. They are generally horizontal, and those which deviate from the horizontal are the deepest ruts. Their dip is uniformly from the ENE., or towards the WSW.

The parts of the rock most scratched are the lowest.

This knoll, in the part of it cut through by the railway, slopes down towards the north; that is to say, the high ground is all to the south, and there is still lower ground adjoining it on the north.

The rock is a sort of porphyry or tufa, which is full of irregular crystals or concretions of red jasper. These hard concretions, where they occur on the smoothed parts of the rock, shew that they have been worn down by the smoothing agent wherever they were exposed to its operation. There are places where the smoothing agent has not apparently acted so strongly, or at all, on these hard concretions, These are the parts of the rock which face the S. and SW., indicating, therefore, that the rubbing and smoothing agent had passed from the N. or NE.

Over this knoll there are several large boulders of greenstone, and a multitude of small stones sticking promiscuously in the well known clay usually characterised by them.

Above this deposit, there are stratified beds of fine clay lying horizontally.

About 100 yards nearer Linton, there is another ridge of trap, which has been cut through for the railway. Its NW. face exhibits also a smoothed and polished surface, about 8 or 9 feet square, with numerous horizontal striæ.

These smoothed rocks are at a height of about 90 feet above the sea.

It is proper to mention here, as bearing on the nature of the agent which passed over and smoothed the rocks described, that all the trap ridges in the neighbourhood of Linton are bare on their north sides, and have *tails* of clay and gravel on their south sides. In particular, there is a remarkable vertical dyke of porphyry, called Smeaton Hughs, running by compass E. and W., the north side of which presents, for several hundred yards, a perpendicular wall from 30 to 50 feet high, and which on the south side has an accumulation of debris which conceals its south side, even to the top.

I may here, also, allude to the recent discovery, in Fife, of trap rocks, smoothed and furrowed, in a way precisely similar to those at Arthur Seat and Linton. I have not, myself, seen the rocks, and am indebted, for the following description of them, to Mr R. Chambers. The discovery took place in improving the road between Burntisland and Kinross. A cut for that purpose was made in the crest of a low range of hills looking to the north-west, over the widespread morass around Beath Kirk. At this point, the ridge consists substantially of a thick bed of greenstone, dipping to the north-west, at an angle of about 40°. At the base of the exposed rock, some diluvium, surmounted by the sandy deposits of a mossy lake, may be seen. The surface is straight-faced, and bears the manifest appearance of having been polished and furrowed by some mechanical agent. This agent has acted in a steady and uniform manner, for the polishings and furrowings are all in one direction. It is remarkable, however, that they dip to the SW., at an angle of about 25°.

It appears that this smoothed and furrowed rock is not in any mountain gorge or valley, but is a projecting ridge which stands out facing an extensive morass. Close by, in a hollow produced by two prominences in the original rocky surfaces, we find a mass of diluvium in its original state. Immediately over the basis of the marked rock surface, there lies a sloping mass of recomposed diluvium,—that is, mixed up with sandy and other stuff, as well as boulders of greenstone, mica-schist, &c. Over its surface lie horizontal beds of impure sand and peaty matter, the deposit of a lake now degenerating into a morass.

At Blackford Hill there was, till lately, a considerable surface of smoothed and striated rock, of which an account will be found in a Memoir published by me on the Mid-Lothian Coal-fields.

On the knoll occupied by Craigmillar Castle, there were similar dressings seen, as I am informed by Mr Jardine, when the rock was quarried a number of years ago. In several places near the castle, the conglomerate rocks are much rounded, and the bared surfaces all towards the north-west, but no furrows are at present discernible.

3. The next circumstance, which I deem worthy of notice here, is the rounded form or shape of the rocks on the NW. side of Sampson's Ribs, and the comparatively sharper or more angular forms of the rocks on the S. and SE. sides of the same hill.

II. Having described the most important facts connected with the appearances of smoothing and rutting on the rocks of Arthur Seat, and on others in the neighbourhood, I proceed to the more difficult question, What is the agent which has produced them?

1. In the first place, it appears clear, from some of these facts, that *an immense volume of water has passed over the country—at or immediately after* the period that these rocks were smoothed and striated.

There are two points involved in this proposition, *first*, the passage of the water; and, *second*, the time when the rocks were so operated on.

(1.) As to the first point, no doubt can be entertained. The accumulation in Sampson's Ribs gully, of limestones, coal, sandstones, felspar, porphyry, greywacke, and other rocks, which do not belong to this neighbourhood, and must have been brought from a distance, is one unequivocal proof of this.

The deposit of gravel and sand at Windygowl, about a quarter of a mile to the eastward, is another clear proof of the existence of running water at that level.

Nor would it be difficult to adduce indications of water currents at a still higher level on Arthur Seat. Mr Maclaren[*] gives an account of rolled blocks and boulders in places adjoining this gully, and from 300 to 400 feet above it.

And that the water which flowed at this high level must have had great force and power, is manifest from the enormous size of some of these boulders.

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[*] Geology of Fife and Lothians, p. 47.

I am aware that some glacialists have ventured to explain the transportation of boulders, even in the situation of those now referred to, by imagining that they were transported on ice-floes, and dropped on the places where they are now found. Mr Maclaren mentions several facts inconsistent with this theory as applicable to Arthur Seat. He states that some of the boulders are of sandstone, and on places "400 feet *above any spot where sandstone now exists in situ.*" But if the parent rock was below the level of these boulders, how could they have been floated from it to their present position? He states, also, that the accumulations of boulders are almost always on the east side of prominences, leading to the conviction, that they had been hurried along by a rush of waters from the westward, and deposited on the lee-sides of these prominences.

The occurrence of limestones, sandstones, shale, and coal, in the boulder-clay of the gully at Sampson's Ribs, is equally adverse to the theory of flotation by ice-rafts. Wherever these blocks have come from, they must have been brought from parent rocks, situated below the level of Sampson's Ribs. Burdiehouse, Kirkliston, Mid-Calder, Queensferry, and Newbigging in Fife, are the only places where the limestone with *Entomostraca* is known, and they are all of them at a lower level. Gilmerton, which is the nearest point at which marine limestone is worked, is still lower, and so also are the other more distant localities from which it probably came, viz., Linlithgow, Charleston, and Invertiel in Fife. The coal-sandstone and coal, being exceedingly friable, could not have been brought from a great distance; and there is no place in the neighbourhood from which these blocks could have come, which is not, at least, 200 feet below the level of Sampson's Ribs.

But these boulders, if they had been dropped from floating icebergs, would not have been imbedded in the extraordinary deposit characterised by them. If it was the bottom of the sea upon which they fell, one would expect to find them lying on beds of sand or mud; whereas they are generally enveloped in a tenaceous clay, in some places 100 feet deep,—as at Leith. Was this clay also transported and dropped by individual icebergs? If so, would it form, as it does, a continuous deposit over the whole of the Scottish Lowlands? Would it not rather have occurred only in small patches, and presenting some traces of stratification?

The same agent which brought the boulders, must have brought the clay in which they occur; and, spread as it is over the whole of the country south of the Grampians, there must have been one agent, which transported and spread it,—not separate icebergs floating here and there.

(2.) The next point, is at what period this debacle of water took place?

The clay and boulders are in contact with the smoothed and striated rocks. There is seldom any intervening deposit. I have occasionally seen a thin layer of sand between the boulder clay and stratified rocks. There are several localities pointed out in my Memoir on the Lothian Coal-fields. At the part of the Victoria road, about 300 yards to the north of Sampson's Ribs, Mr Nichol pointed out to me a layer of sand, about an inch or two in thickness, at irregular intervals, between the boulder clay and the sandstone strata. These sandstone strata presented no appearances of smoothing and rutting. Their original covering of sand had apparently not been altogether removed and forced away by the rush of boulder clay. That might happen in some places and not in others; but wherever there are rocks smoothed and striated, the boulder-clay is in immediate contact with *them*.

I do not say that this circumstance, of itself, proves that the dressing of these rocks is to be attributed to the passage over them of the boulder-clay, loaded with its heavy and compact greenstones and felspar porphyries; but it is a very important step towards the solution of the question, to find, that instantly after these rocks were smoothed and striated, they were covered by this deposit. Not only is there nothing to indicate the existence of any interval of time between the two things, but some of the striæ are so minute, that had they not been covered up almost so soon as formed, they would not have been preserved.

2. But would the passage of this boulder-clay, forced forward by an enormous or powerful volume of water, have *produced the smoothings and striæ* on the rocks?

That water, even by the friction of its own particles, will, in time, wear down the hardest rock, is undeniable; and if it is hurrying along clay and gravel and heavy stones, the process of attrition will be great and rapid. I think, therefore, that it is not difficult, in this way, to understand how rocks (whether large boulders or hill-sides) have been worn down, and smoothed and polished.

But how are the *scratches* and *furrows* to be accounted for? Ordinary gravel, however great the accumulation, would not produce that effect. But blocks of porphyry, basalt, felspar, and other hard stones, if heavy and angular, would, I think, be graving-tools quite sufficient for the purpose. Now, as already stated, there are multitudes of angular blocks of porphyry, and other hard traps, in the clay deposit covering these scratched rocks, many of which were seen to be in actual contact with their surfaces when first exposed by the workmen; so that the instruments necessary for the purpose were lying on the spot where they had been at work, and in the farther execution of which they had been arrested.

That water has been the *polishing* agent, will be the more readily allowed by those who are familiar with the polished surface of rocks forming the channels of mountain torrents; and were any great debacle to take place, sufficient to hurry over these surfaces not merely sand and gravel, but also hard blocks a ton or more in weight, I have no doubt that abundance of ruts and scratches would be formed on them.

That water was the agent in this case, is the more clear, from the following considerations:—I have already alluded to the natural rock fissures at Sampson's Ribs, and to the absence, in many places, of a portion of the rock on the south side of the fissure. The probability is, that these south portions of the fissures were torn or broken off by the force of the boulders in passing through the trough. But the point to which I wish more particularly to direct attention is the fact, that in very many cases, the angle formed by the north side of the fissure and the east side of the gully is rounded off, as shewn at *b c* in fig. 2; and not only so, but occasionally the rock along the north side of the fissure itself has been smoothed; that is to say, whilst the agent (whatever it was) was passing mainly in a direction from NW. to SE., and smoothing the wall-faces of the trough or gully, this agent must have entered so far into this fissure, and others in the same manner, in order to smooth their faces. The amount of divergence from the general axis of the gully and direction of the moving and smoothing body, must have been from 20° to 30°. Now, I can conceive no agent but water which could act in this way.

There is another circumstance which leads to the same conclusion. I had the good fortune to find, in company with Mr R. Chambers, a small trough of rock, the sides of which, when cleared out and washed, were seen to be smoothed and furrowed in a manner precisely similar to those in the larger trough or gully. This little trough was only 4 feet in length, its width at the top or widest part was only 1 foot, and its depth not quite 2 feet. It was situated about 80 yards to the north of the principal gully, and its direction was NNW. by compass, so that this trough was evidently connected with, and a part of, the phenomenon. Now, its dimensions were so small, that no other agent than water can be conceived to have formed it.

3. There is another question, in some respects subordinate, but still bearing on the facts very materially, viz., from what *direction* did the rush of waters come ?

At Sampson's Ribs, the appearances indicate a NW. direction; for it is the NW. side of the hill on which these smoothed appearances occur, and it is the SE. side where there is an accumulation of sand and gravel, indicating a less tumultuous state of waters, as if on the *lee* side.

On this hypothesis, it is easy to explain the direction of the scratches on both sides of the gully, and, in particular, the slope of the deepest ruts upwards to the SE. To the north of the gully the ground slopes to the north, so that, if a rush came from that direction, the blocks carried along by it, would move upwards to the south, and leave marks of that direction on the sides of the wall-faces as they rushed past them. It is only at the north part of the gully that these ruts, deviating from the horizontal, and dipping to the north, occur.

Some of the scratches, after persevering horizontally for 18 or 20 inches, suddenly indicated a bending downwards, and a greater faintness of marking, at their *eastern* extremities. The blocks being pressed on the wall-faces by the onward current, would, of course, produce ruts corresponding with the line of the current, only so long as there was sufficient pressure on them ; but the pressure could not be exactly continuous for any length of time ; and as it diminished, these blocks would, by their own weight, droop, and thus produce the appearances observed.

But whilst the direction of the rush of waters at Arthur Seat and the neighbourhood, seems to have been from the NW., the appearances at Linton indicate a direction from the north or NE.; for a current from the NW. would not easily have smoothed the rocks there, which are smoothed and striated on their faces fronting the SE. The locality in Fife referred to, would indicate a movement from the NNW.

Farther, on this point, I may mention, that between Musselburgh and Joppa, one of the boulders bears two sets of scratches,—one set running NNW., and the other W. by S., by compass, the former having been partly obliterated by the latter, and therefore prior to it.*

This, however, is a very subordinate point, and immaterial to the great question which now agitates geology, whether it is by aqueous or glacial action that the phenomena of smoothed rocks and transported boulders are to be explained.

Without wishing to deny that, in some situations, glaciers may have produced the effects, I certainly do deny that there is as yet the slightest evidence of their existence in the Lowlands of Scotland, and maintain, that the smoothed and striated rocks of Arthur Seat and Markle Ridge, and the boulders which occupy the clay covering of these rocks, are all to be ascribed to the action of an impetuous torrent of water.

No glacialist who saw the gully at Sampson's Ribs, or the knolls of Markle Ridge, would venture to say, that a glacier could have existed at either place. There is no valley in which a glacier could be confined, no mountain to be the birthplace of its flowing ice-streams ; and if a sheet of ice could be supposed to have moved down from Arthur Seat, it would have passed over and across the gully of Sampson's Ribs, and not through it.

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* The boulders which are rutted on their upper surface, must have stuck in the clay, whilst the rest of the debacle moved over and passed them.

4. There is another question, connected with this subject, of the deepest interest in geology, viz., *At what level did the sea stand*, when the rush of waters which brought the boulder clay, took place and smoothed the rocks ?

The answer to this question would open up another field of inquiry, too wide to be entered on in this paper, though it is a subject closely connected with the results to which we have been brought.

The facts recorded in this paper, are probably too scanty to furnish a satisfactory answer. But, they do go pretty far to shew, that when the boulder clay was deposited, the sea stood at a level much higher than Sampson's Ribs. In the Memoir on the Lothian Coal-fields before alluded to, it is attempted to be shewn, that it stood greatly above the top of Arthur Seat.

The horizontal beds of sand and gravel, and of the sloping but stratified bed of coarse gravel, on the SE. side of Arthur Seat, indicate very clearly the existence of a sea which stood more than 400 feet above the present level;—though it is difficult to say, whether these beds were deposited before or after the arrival of the Boulder clay.

Farther, it is probable, that since these deposits took place, there has been no material change in the relative levels of the country. What is the surface of the present dry land, was then the bottom of the sea, consisting of the very same hills and hollows, plains and slopes, as are now covered with terrestrial vegetation. On this principle, and apparently on no other, is it possible to explain the rounding and smoothing of the existing hills and knolls, on their north and north-west sides ; the bed of coarse gravel at Windygowl, which has the same slope as the surface of the hill ; and the identity of the boulder clay at the top and bottom of Sampson's Ribs.

After the tumultuous waters which transported the Boulder-clay were assuaged, the upper parts of that deposit were acted on by the natural currents and tides of the ocean ; and in all the hollows, accumulations of stratified mud took place, destined, after the land rose out of the sea, to be the sites of brick and tile works. In this way the boulder clay would everywhere be abraded to a considerable depth; and, hence, there is generally found on its upper surface, great quantities of boulders of all sizes.

I refrain in this paper, which was intended merely to describe and account for a particular phenomenon, from entering farther on these large and difficult questions. But, I cannot conclude, without expressing the gratitude which I feel, in common with every geologist, to Mr Nixon, the inspector of works in the Queen's Park, under the direction of the Commissioners of Woods and Forests, for agreeing to protect and preserve from injury, some of the polished and striated rocks at Sampson's Ribs. They were in the way of the new carriage-drive which Mr Nixon had lined out, and were intended to have been entirely removed. But I had simply to request the preservation of a portion of these rocks, and explain the reasons of it, to obtain from this gentleman a ready and courteous compliance, though involving some change of his plans. I only hope, that he will not put over these rocks a sign-board, to inform the public, that they are "*Glacial Rocks*,"—which is the term he applies to them, in his correspondence with me on the subject.

On producing White or Neutral Light, by means of ordinary Artificial Light. By GEORGE TAIT, Esq., Advocate, Vice-President of the Royal Scottish Society of Arts. Communicated by the Royal Scottish Society of Arts.*

It is generally known that ordinary artificial light, such as that obtained by the combustion of wax, tallow, oil, or carburetted hydrogen gas from coal, produces a tinge of colour very different from that produced by the white light of the sun. This is found, upon examination, to be an orange tinge, so exceedingly strong as to reduce it to orange colour, slightly modified by blue, as will be seen presently in detail. The two lights may be compared, in a general way, by observing the colours of different objects enlightened by them separately at the same time; for example, throwing contiguous shadows of an opaque body from each of them upon a white surface, and comparing the colours of the shadows, the shadow from each light shewing the colour of the other light which alone falls upon it; the white, however, if not in large proportion, assuming a bluish appearance from contrast with the orange all around, of which blue is the complementary colour. This orange tinge necessarily affects very much the appearance of the colours of the objects exposed to artificial light. And it would be of importance for some of the useful arts, in which it is necessary to distinguish colours, for the department of painting in the fine arts, whether for executing or for viewing pictures, and for other purposes generally, if light free from tinge and of sufficient strength could be produced by means adapted for ordinary use. And, accordingly, this subject has, upon my suggestion, been inserted by the Royal Scottish Society of Arts among those recommended by them for consideration.

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* Read and exhibited before the Society, 23d November 1846.

This could, perhaps, be done to most advantage by a pure white flame, if such a flame can be produced by convenient and unexpensive means, and without deleterious effects on the air, other than those necessarily produced by combustion.* Or, failing that, it might be done, although not so conveniently or satisfactorily, by blue flame, if producible on the conditions just mentioned, used along with ordinary orange flame, in such ratio as to neutralize it, on the principle to be immediately noticed. In the mean time, I submit to the Society a few observations on the application of a simple and obvious method of effecting the same object, by using any of the ordinary artificial lights, and absorbing the excess of orange by interposing a transparent medium of a proper colour. I had felt very strongly the bad effect of ordinary artificial light on pictures, in using portable dioramas, which I lately submitted to this Society, which led me to endeavour to devise a remedy; and I have found it to be completely obviated by the application of that method, in the manner which I shall now explain.

Artificial light is most conveniently obtained by using carburetted hydrogen gas, such as that made in a proper manner from parrot or cannel coal; and a powerful light is easily obtained in this manner. Gas from *common* coal is much inferior. Oil is not so convenient or so cleanly as gas. Candles are inconvenient from their varying in height, which, for many purposes, renders frequent adjustment necessary; and when they are enclosed in the manner which is generally necessary, even the least fusible are liable to melt, although they have as large a supply of cool air as can conveniently be given, unless there be very few of them, which makes the light very weak. The *expense* of different lights will be noticed presently.

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* The "Bude Light," which is produced by a stream of oxygen within a flame of carburetted hydrogen, such as coal gas, and which is very brilliant, has still so much of the orange tinge as to render it unfit to be used unmodified, as being white light; and, even if it were white, it is much more troublesome and expensive than the method of obtaining white light to be now proposed.

The artificial light is to be inclosed in a box or lantern of a suitable size and construction, or otherwise, as circumstances may require, and in such manner that none of it may escape by the openings for admitting and discharging air or otherwise, except what is modified by passing through the coloured medium. The modified light may be used either by allowing it to radiate directly upon the objects to be enlightened, or, in the case of transparent paintings, or the like, by transmitting it through them. I have used it satisfactorily in both ways.

As the Society are aware, the sensation of colours is caused by objects reflecting or transmitting colours produced by the rays of light to which they are exposed. There are three primary colours, yellow, red, and blue; of two, or all of which, combined in various ratios, all other colours are composed. Light, with very rare exceptions, is compound or heterogeneous, producing two or all of the primary colours. The pure light of the sun produces them in such ratio, that when wholly or proportionally reflected or transmitted, they neutralize each other, and produce white or neutral grey (which is merely a shade of white, having no predominant colour); and light producing them in such ratio is often called shortly *white* or *neutral light.* In 100 parts of white, there appear to be about 18 parts of yellow, 32 of red, and 50 of blue, all of equal intensity, or nearly as 3, 5, 8.* When two only of the primary colours occur, yellow and red, yellow and blue, or red and blue, they form the secondary colours, orange, green, or purple ; and when they occur in the ratio in which they are produced by white light, they form perfect or normal secondaries. The colours of those objects which are called coloured, is caused by their having the quality, from their peculiar structure, or otherwise, of reflecting or transmitting different parts of the rays forming the compound light to which they are exposed, the remainder being absorbed and extinguished. When light is composed of rays which produce colours in a different ratio from that in which they neutralize each other, the colour of the excess is produced on a white surface, and the appearance of coloured objects is affected according to the excess or the deficiency of the rays producing particular colours. For example, an intensely blue object exposed to a perfect orange light appears black, as there are no blue rays which it may reflect or transmit, and it absorbs and extinguishes the red and the yellow. If some rays producing blue be combined with the orange light, this effect is modified according to their quantity.

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* Field, by mixing pure coloured liquids of equal intensity, and also by combining hollow wedges of glass containing such liquids separately, and transmiting daylight, found the ratio of the component parts of white to be 3 parts of yellow, 5 of red, and 8 of blue. (See *Chromatics*, by George Field, 1845, pages 224, 231–2; *Chromatography*, by the same, 1835, p. 247.) Hay, by mixing pure powdered pigments of equal intensity, found the ratio to be 3 parts of yellow, 6 of red, and 9 of blue. (See *Nomenclature of Colours*, &c., by D. R. Hay, 1845, p. 8; *The Principles of Beauty in Colouring systematized*, by the same, 1845, p. 17.) The first series of numbers contained in the text, are the average of those three sets of observations; which coincide, with the exception of a slight difference in the ratio of the yellow and the red, arising perhaps, from the difficulty of distinguishing whether either of those colours is entirely free from a tinge of the other.

In ordinary artificial light, the rays producing red and yellow appear to occur nearly in the same ratio in which they occur in white light. And as there is an excess of them over the rays producing blue, all that is necessary in order to produce white light, is to interpose a transparent medium of such a depth of blue as to absorb that excess, allowing such a portion of them to pass as is necessary to neutralize the rays producing blue, all of which have been allowed to pass.

It appears that that excess may be best absorbed by interposing coloured glass, of a proper depth of blue, according to the ratio of orange in the flame. It ought to be "coloured" glass, that is, coloured in the melting-pot. Glass "stained" blue, that is, partially coloured by applying the colouring matter to the surface, and fixing it by a heat sufficient to soften the glass, but not to melt it, is not transparent ; in which respect blue differs from other stained colours in general. It is very difficult to get "coloured" glass made of any precise tint. Perhaps the tint required could be conveniently produced by having the glass made at first a little too dark, by making it a little thicker than necessary, and afterwards reducing it sufficiently by grinding, and, if necessary, by polishing.

If "coloured" glass of the proper depth of blue cannot be obtained, coloured glass of a lighter blue, or colourless glass, may have the necessary colour applied to it with transparent paint. I have found the blue commonly called French blue or factitious ultramarine, to answer the purpose very well. It is very transparent, but sometimes has a slight tinge of red. Genuine ultramarine, which is prepared from the precious stone *lapis lazuli*, and which is very expensive, at least the best that I could procure in Edinburgh as such, however much superior it is for ordinary painting, is much less transparent, and has a dusky slaty appearance by transmitted light. Cobalt is still more objectionable in both respects. Prussian blue is transparent, but produces a considerable tinge of green, which it is necessary to neutralize with red, the consequence of which is a considerable shade of grey.

The French blue has this disadvantage, that till the oil and the varnish used in applying it are completely dry throughout (which in the case of the oil may be very tedious), there is a very wide refraction and dispersion of a large portion of the rays producing red; which is easily observed by holding the glass close to a flame. The consequence of this is, that when a limited beam of light is thrown on a perpendicular object, there is an offensive green tinge in the direct line of the light, caused by the blue and the yellow not being neutralized by that portion of the red which has been refracted. This consequence does not take place where the light is used in a diffused manner, for example, a flame surrounded by blue glass, because the whole of the component parts of the transmitted light reach every object, either directly from the flame through the intervening part of the glass, or by refraction from other parts of it. The refraction of red takes place also with Prussian blue having the green tinge neutralized by cochineal lake. It does not take place with genuine ultramarine or with cobalt; but their want of transparency and the dulness of their colour by transmitted light, render them unsuitable for this use. "Coloured" glass is free from the defect of refracting red.

The paint ought to be applied so as to be as transparent as possible, unless it be intended to be dim, in order to diffuse the light.

It is most conveniently applied with oil, which ought to be as free from colour as possible, and to be made very drying. As it dries slowly, and ought therefore to be used sparingly, mastic varnish or other varnish may be used along with it to dilute the paint. But the oil of turpentine, or the alcohol of the varnish, go off so quickly that the fluidity of the paint varies very much, and it is difficult to apply it smoothly and uniformly of the proper tint. Or, without using varnish, gum mastic may be dissolved in the oil by heat, at as high a temperature as can conveniently be used in painting the glass, perhaps 70° or 80°, or more, which makes much less oil necessary than would be required at a lower temperature. By applying the paint in several coats, allowing each to be thoroughly dry before applying another, instead of applying one thick coat, the precise tint required is more easily produced, and the drying of the oil is promoted. If the paint, when finished and dry, be deficient in transparency, a coat of mastic varnish or other varnish may be applied, but cautiously, so as not to dissolve and disturb the paint.

Another method is, without using oil or varnish, to mix the paint in the state of powder with gum mastic melted by heat, which requires to be above the boiling point of water, and to keep up the necessary temperature while painting. By this method, the paint is fully as transparent as by the other, it becomes hard immediately upon cooling, and it does not refract red.

In order to adjust the depth of tint of the glass, whether "coloured" or painted, the white light of the sun may be received on a piece of white paper, placed at a short distance behind a window, which is to be used as the standard of white light. The paper ought to be as purely white as possible, and a blue tinge ought, in particular, to be avoided. The light may be conveniently obtained by excluding the external light from the apartment, leaving an opening of a proper size near the lower part of the window, and covering the opening with purely white tissue paper, which receives the external light, and transmits the greater part of it, so that a portion of it is received upon the standard paper. The light received upon the tissue paper may be either the direct rays of the sun, or diffused light from white or neutral grey clouds or less condensed vapour, or during rain or fog, but not from blue sky. In either case, the observation ought not to be made when the sun is low, on account of the warm tinge which the light usually has at that time, or when there is a tinge from smoke. The tissue paper may be dispensed with, and diffused light may be received directly from without upon the standard paper; but the method before described is, in general, more convenient and satisfactory.

Another piece of similar paper is then to be exposed to the artificial light transmitted through the glass, being, at the same time, completely shaded from the day light; and, if "coloured" glass be used, it is to be procured or made of such a depth of blue that the latter paper may appear as white as the standard; or, if painted glass be used, it is to be brought to produce the same effect, making allowance for the temporary green tinge before referred to. The more correctly that this paper is made to agree with the standard, the better. A greater degree of blueness is, in particular, to be avoided. The two papers are more easily compared if they be made both to receive the same quantity of light.

If, from the flame and the paint used not correctly neutralizing each other, or from any other cause, there be a little of a permanent green tinge, it may be neutralized by the addition of a little transparent red; or, if purple, by transparent yellow. The same principle applies to the adjustment of the proper tint of "coloured" glass.

If the light pass obliquely through a glass, or part of a glass, less colour is, in consequence, necessary.

After the paint is completely dry, the light transmitted ought to be again compared with white light, and any little inaccuracy corrected.

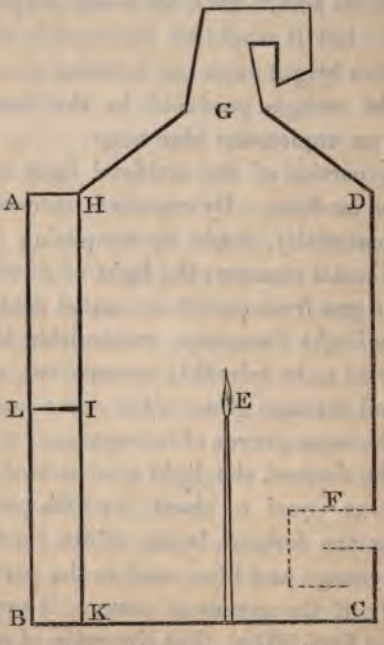
When one glass has been adjusted, others, whether "coloured" or painted, may be adjusted by comparing their effects with its effect, by artificial light.

If the paper for which a glass has been adjusted have a slight tinge of colour, paper of the same tinge ought always to be used along with that glass, when used with a similar light.

The excess of orange is greater as the flame is less bright; for example, greater in a swallow-tail flame of gas from parrot or cannel coal, than in the flame of a well-constructed argand lamp with such gas or fine oil; greater in the flame of a wax or composition candle; and still greater in a duller flame, such as that from coarse tallow or common oil; as may be easily seen by comparing the colours of contiguous shadows from two lights on a white surface; so that a glass adapted for a bright flame is not accurately suitable for one less bright. At the same time, the excess of orange which would thus be transmitted if the flames do not differ materially in brightness, is comparatively so small, that, for many purposes, it might be disregarded; but it would be undesirable to use a glass adapted for a less bright flame, as it would absorb more than necessary of the orange produced by the flame used, and would produce an unpleasant blue tinge.

A very large portion of the artificial light is absorbed by the blue of the medium. By repeated observations, which did not differ materially, made by comparing (by means of shadows in the usual manner) the light of a bright swallow-tail flame of the gas from parrot or cannel coal, made by the Edinburgh Gas-Light Company, transmitted through glass painted blue, so as to be tolerably transparent, with the same light transmitted through glass of the same quality, brought to apparently the same degree of transparency without colour, I found that the former, the light transmitted through the blue medium, was equal to about one-fifth part only of the latter. And as the former, being white light, consists of equal parts of orange and blue, and as the portion absorbed consists entirely of the excess of orange, it appears (if that portion be about four-fifths), that the ratio of orange to blue in such a flame is about nine times that in white light. The portion now stated as being absorbed, is given merely as an approximation. It could be ascertained more accurately, by comparing the artificial light transmitted through "coloured" glass with that transmitted through colourless transparent glass of the same quality. There is rather a larger portion to be absorbed in a duller flame, as has just been remarked. The very great excess of orange thus ascertained to exist in artificial light, accounts for the very great difference between its effect and that of white light upon the appearance of the colours of objects, and may give some idea of the extent of that difference to those who have not seen them distinctly contrasted.

I shall now exhibit white or neutral light and ordinary artificial light in contrast. This may be done in different ways. It is done conveniently and distinctly, by transmitting the lights through paper, or other pellucid substance, by means of the simple apparatus represented by the following diagram. It is, of course, to be used in a dark apartment.



A B C D, box of tin-plate of such size as may be necessary, bright within, opening at the front, A B, and the back, *C D.*

E, horizontal line of flames parallel to the middle of A B, and about half the height of A B, behind it, to diffuse the light. It has already been remarked that gas is the most suitable light, and that candles are the least so.

F G, openings across the box, for the admission and discharge of air, blackened within, and formed so that light may not escape.

H I, blue glass across the upper half of the box and a little back from the front, so that the inequalities of the paint on the glass may not appear on the front paper.

I K, plies of tissue, or other white paper, immediately below the glass, to reduce the light there to the same intensity as that transmitted through the glass.

I L, thin horizontal partition, almost touching the front paper, to keep the two lights separate.

White papers, moveable, are stretched across the front A B, having colours or sketches painted upon them to shew the contrast of the two lights, the upper half of the papers exhibiting white light, the lower half ordinary artificial light. The papers ought not to be thinner than " thin post." It is better that they should be thicker if the light be sufficiently strong.

One paper which I shall exhibit, has vertical stripes of white, and of the primary and the secondary colours, which I painted by daylight with transparent water-colours, each stripe of colour being light upon one side and gradually deepening moderately to the other side.

On looking at those stripes, and keeping in view that white light gives the natural appearance of colours as in pure daylight, it will be observed, that, by ordinary artificial light, all is involved in the exceedingly strong orange tinge before described, and the appearance of the colours is altered accordingly; for example, *white* and *yellow* become hardly distinguishable from each other. *Orange*, if pale, becomes not distinguishable from yellow. If more intense, it becomes a little warmer. *Red*, if pale, becomes hardly distinguishable from yellow. If more intense, it becomes less or more orange, and ultimately verges toward red. *Blue*, if of a certain intensity, becomes a pure neutral grey, the blue being neutralised by the orange of the light. If less intense, it becomes a dull orange, being the excess of the orange of the light combined with the neutral grey produced by the remainder of the orange combined with the blue. If more intense, it becomes of a very dark grey, slightly tinged with the excess of blue. *Green,* if pale, becomes of a dull orange, less or more tinged with grey. If more intense, so that the orange of the light may be neutralized by the blue of the green, it becomes of a dark muddy tinge, more or less of a yellowish cast. *Purple,* if pale, becomes hardly distinguishable from orange. If more intense, so that the orange of the light may be neutralized by the blue of the purple, it becomes of a dark muddy tinge, more or less of a reddish cast. The appearance of all the more compound colours is altered in the same manner; the white forming light tints becoming orange; the blue entering into the various compounds of green and purple being almost lost, from the want of blue in the light; and the red and yellow entering into compounds, being much heightened.

The difference of effect of the two lights will perhaps be more striking, by now exhibiting on the front of the box, another paper, on the upper and lower halves of which I painted with transparent water-colours, by daylight, two sketches precisely similar to each other. It will be particularly observed how much the blues and the compounds of blue are destroyed.

It is thus obvious how very much ordinary artificial light is inferior to white light, or rather how unfit it is to be used, wherever it is necessary to distinguish the colours of objects, or desirable to perceive them in their natural pleasing and harmonious appearance.

In obtaining white or neutral light by the method now proposed, there is of course a loss of a large portion of the artificial light. This loss consists partly of the light which would be lost by reflection and extinction in passing through colourless transparent glass, except that there is no reflection from the glass on the side which is painted, instead of its being from both sides as usual; which loss appears, according to observations made by Sir William Herschel, to vary from about one-thirtieth to about one-fifth part, according to the quality of the glass, from pure colourless plate-glass or white flint-glass, downwards.[*] But if the angle of incidence be reduced to 60°, there is a perceptibly greater loss of light, which increases rapidly as the angle is farther reduced;[†] and, therefore, such obliquity ought to be avoided. It consists also, in the case of painted glass, of the light lost by defect of transparency, which, of course, varies according to the execution. But it consists chiefly of the great excess of orange in the light not stopped by the two preceding causes, which is absorbed by the blue of the medium as before noticed. The total loss in any case is easily ascertained by means of shadows in the usual manner. But it appears that by using a bright flame and glass of good quality, "coloured" or well painted, the loss should not exceed five-sixths, or at most six-sevenths. It may be less.

The loss of light, of course, causes a corresponding increase of expense. But it appears that the objection on this ground is not so great as might be anticipated. Dr Fyfe states the following among the results of observations made by him with regard to the expense of a variety of materials for producing light, taking as the unit the expense of good coal-gas, yielding 12 per cent. of matter condensible by chlorine:—Gas argand, 1·00; gas swallow-tail, 1·40; solar oil in solar lamp; 2·00; solar oil in common argand lamp, 3·98; whale oil in do., 5·00; sperm in do., 8·00; moulded candles (short sixes), with two wicks, 12·70, with one wick, 13·50; wax candles, 25·90; Spermaceti and composition candles, 29·20.[‡] So, that, supposing the expense of white light to be six times that of ordinary artificial light, it may be produced by a gas argand lamp for little more than the expense of the same quantity of orange light from common whale oil in an argand lamp; for about half the expense of tallow candles; and for less than a fourth part of the expense of wax candles; and if gas cannot be obtained, the solar lamp may be used, which will produce white light at the same expense as orange light from tallow candles, and at less than half the expense of wax candles, independently of the diminution of the ratio of orange, in consequence of the great brightness of the flame of that lamp.

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[*] See Table by Sir William Herschel, in Rees' Cyclopædia, "Light."

[†] See Table by M. Bouguer, ibid.

[‡] Transactions of the Royal Scottish Society of Arts, vol. ii., pp. 133–147; Edinburgh New Philosophical Journal, vol. xxxii., pp. 221–235.

The want of *intensity* of the light, as thus modified by the blue medium, is, to a certain extent, a defect for some particular purposes. But, for general use, the intensity of artificial light is often subdued and diffused by ground glass or other means used expressly for that purpose. And any quantity of neutral light may, of course, be obtained by using a proportionably greater quantity of flame; and, if necessary, it may be concentrated by using lenses, or metallic reflectors properly constructed and applied; the use of which, indeed, where applicable, may render a great increase of flame unnecessary. And I am not aware of any other means so convenient and unexpensive of producing the uniform neutral light, or artificial daylight, so desirable for many purposes.

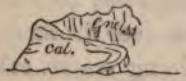
I submit these imperfect hints with diffidence. But, perhaps, they may be of this use, at least, that they may tend to draw the attention of more competent persons to the production of white or neutral light.

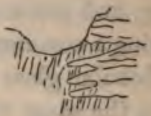
EDINBURGH, 1846.

Postscript.—Since writing these observations, my attention has been directed to a short popular treatise " On the Influence of Artificial Light, in causing impaired Vision, and on some Methods of Preventing or Lessening its Injurious Effects on the Eye," by the late Dr James Hunter of Edinburgh, published in 1840, of which I was not aware, and which appears to be a judicious and intelligent work. One of the methods recommended by him, is founded upon the sound and obvious principle of correcting the excess of orange in artificial light, in order to produce white light, or something near it. But, although the means proposed by him for effecting this may be sufficient as palliatives for medical purposes, they appear not to be suitable for the purposes of art. One of those means is to use, over an argand flame, or a flat flame, a conical reflector, the inside of which is without gloss, and is stained or painted of a tint of blue, of which he gives a specimen, in order to absorb the orange, and to throw down the blue rays to qualify the orange light below. But upon trying it over an argand flame, with the tint prescribed, and also with a darker tint, I found that the orange tint below was not materially subdued. He also proposes the absorption of the excess of orange by transmission through a blue medium. One of the modes proposed, is to have the chimney of an argand lamp-stand of a "very pale blue colour," and to throw down the light transmitted through it by a bright conical metallic reflector. But this description of the tint of blue is very indefinite, and it does not appear to indicate nearly the depth of tint necessary for neutralization, which absorbs so large a portion of the light as before mentioned. Another mode proposed, is to transmit the light of an argand flame, or a flat flame, by a similar reflector, through a disc of glass under the flame, stained blue of such a depth of tint, that a piece of white paper seen through it in daylight, may appear "nearly of the colour of the blue sky," or for wood engraving, and some other such purposes, to transmit the light, having a common white reflector over the flame, through a large globular glass bottle, containing water, coloured according to the same standard. But the intensity of the blue of the sky varies through every tint, from the darkest to the palest, according to the latitude of the place, the state of the atmosphere, or the part of the sky observed, the blue towards the zenith being, in general, many times darker than that towards the horizon. It will be observed also, that, by those modes of absorption, more or less of the artificial light is allowed to escape unmodified. Whereas, by the method which I have suggested, this is prevented; and, by adjusting the colour in unison with white natural light, a standard is at once obtained, which is precise, correct, and invariable, and which secures the production of pure white light fit for any use in the arts.

Remarks on the Geological Relations of the Gneiss of the Alps. By Professor B. STUDER of Berne. (From a Private Letter addressed to Professor J. D. Forbes.)

"I think that I have obtained a precise and decisive result with respect to the great question of the stratification of gneiss, which has so long occupied me. From what we saw at the Mettenberg and in Urbach, it is perfectly evident, that the direction of the laminæ of gneiss is independent of what is called stratification, that is to say, that it is not an effect of gravity, that these slaty rocks have not been elevated from a horizontal position into the vertical one in which we now find them; that the great mountain masses with a fan-like stratification have acquired this structure as an effect of crystallization, or of other forces independent of gravity. You are aware that the calcareous mass of the Mettenberg forms a wedge in the gneiss, and that, in the valley of Urbach, several wedges of limestone and gneiss are fitted together; but in the two localities the slaty structure of the gneiss does not shew the least dependence on the limits or form of these wedges. The dip is to the south, under an angle of 50°, while the limit of the gneiss and limestone is horizontal. To maintain the opinion of the elevation of the gneiss, would be to suppose that this rock was older than the limestone, and that the latter was introduced into the crevices of the mass of gneiss; a supposition which is inadmissible. But if, on the one hand, I am satisfied that the theory of the origin of our mountains of gneiss by the tilting of the strata (*par rédressement*) is erroneous, I am far from being able to give a better one. Supposing the gneiss to be schistose lava, we must be surprised to find so little change in the limestone inclosed in it or in contact, whilst, in contact with granite and other





rocks, we find the limestone pass into saccharoid marble; and we are much embarrassed by so many proofs of an insensible passage from gneiss into sedimentary rocks,—proofs which, of late years, have made the opinion of the origin of gneiss by metamorphism to be almost universally adopted."

On an Economical Mode of using the Potato; in a Letter addressed to Professor Jameson. By JOHN DAVY, M.D., F.R.S., Inspector-General of Army Hospitals, &c.

MY DEAR SIR,—The sad accounts which I read in the newspapers, of the diseased state of the potato, and its consequent decay in most parts of the country at home, and especially in Ireland and the Highlands, bring to my recollection some trials which I made, before I left England, on this root as an article of food, with a view to economy; a few of the results of which I now beg leave to communicate through you to the public, with the hope of a chance of usefulness.

It is known to the chemist that the potato consists principally of starch, of certain juices, chiefly water, impregnated with a variety of proximate principles, and of a cellular fibrous structure in which the starch and the fluid parts are held.

In the ordinary method of making potato-starch, a rude analysis is effected; the greater portion of the starch is separated from the cellular fibrous structure, and also from the juices of the root. As the process is commonly conducted on a small scale for domestic purposes, there is much waste; it is only the starch that is preserved, and not the whole of it, for some of it is retained in the cellular structure: the cellular fibrous part with the juices is either thrown away, or, where a stricter economy is observed, is given to the pigs.

It is to this part, the cellular fibrous, that I wish to call attention, because I believe it to be not without nutritive power, and deserving of care equally with the fecula, the nutritive power of which it may even surpass. I am induced to form this conjecture, considering how low is the estimated nutritive power of starch, and from the circumstance that the cellular fibrous part, when dried, yields ammonia on destructive distillation, and, when incinerated, a comparatively large proportion of the phosphates of lime and magnesia, with some potash and a little silica.

Reflecting thus, I was induced to have the cellular fibrous portion, after the extraction of the greater part of the starch, dressed for trial. It was put into the hands of the cook, with directions to make it into biscuits, baking them in the oven. So dressed, very slightly browned, they were agreeable to eat at the time, and they kept remarkably well. Before I left England I examined one which had been wrapped in paper and deposited in a close cupboard, and it was then good after twelve months' keeping. To give it further and severer trial as to its power of keeping, I brought it with me to the West Indies, and placed it here, in Barbadoes, in a close press, with some other vegetable substances, which had been preserved a long while in England by being merely dried by exposure to the air, cut into thin slices, such as turnip, carrot, and apple. On examining these very recently, after having been nearly a year and a half within the tropics, I found them all, with the exception of the biscuit from the potato, spoilt, being covered with mildew, whilst *it* was hardly perceptibly affected by mildew, and, after exposure for half-an-hour to sunshine, had not the slightest smell of mildew; and moreover, when tasted by a very competent judge, accustomed to the luxuries of the table, was pronounced to be not unpleasant.

Does not the foregoing admit of practical application? Whenever there is reason to suppose that potatoes are infected with the prevailing disease, would it not be judicious, without loss of time, to subject them to the process mentioned,—first, extracting their fecula, which has often been recommended and practised; and next, collecting and preserving as an article of food, by drying and baking, the cellular fibrous portion? Even in favourable years, when there is no risk of the potato rotting, it might often be advantageous to prepare a portion of the potato in this way: it would afford a variety in the form of diet to the poor; the starch would serve to make puddings, and answer all the purposes of arrow-root, for which it is so often sold in the shops; and the cellular parts dressed as a cake with a little sugar, or as a biscuit, might serve the purpose occasionally of bread. It might be useful also at sea on long voyages; and, if mixed with a certain portion of potato-starch, it might be considered as almost identical with the potato itself, possessing, as the mixture would, all the ingredients of the potato not soluble in cold water; and, as regards the starch, its quantity might be so regulated as to be in the best proportion.

I may offer another economical suggestion, which may, I think, be deserving of attention, with a view to avoid waste as much as possible. Potatoes, on being boiled, whether in their skins or without them, yield to the water a portion of starch, and also of a nitrogenous matter, making a weak broth. This broth has a pleasant taste like that of a very dilute animal broth; and tested by corrosive sublimate and tincture of iodine, affords indications of the presence of albuminous matter as well as of starch. Moreover, the extract obtained, by evaporating it to dryness, burns, emitting a smell of animal matter burning, and yields an ash containing a notable portion of potash and phosphate of lime. I may remark that these substances, with magnesia and peroxide of iron, are also found in the potato-skin, in which also they are probably combined with an azotised matter, which may render even the skin likewise nutritive.

In conclusion, may I be permitted to observe, that when famine is threatening a people, ought not attention to be given to every substance of native produce available, capable of affording nourishment, such (to mention a small number) as the acorn, the horse-chestnut, the ground-nut? These contain a large proportion of starch, and the two first mentioned a good deal of albuminous matter. They are not pleasant, it is true, to eat, on account of the presence of a bitter principle, which, probably, may be destroyed by roasting or some other simple process. In the instance of the sheep, the horse-chestnut is found to be an exceedingly nourishing food, and is so used in Switzerland. The nutritive quality of acorns, as the food of swine, is well known. It may seem degrading for man to descend to the food of brute animals; but on the dire event of famine, such a consideration is surely trifling, not worthy of a moment's thought, though deserving of all attention in prosperous and ordinary times, in relation to the welfare and the prevention of the degradation of the working classes;—an attention, unfortunately, which it has not everywhere received, as the existing calamity in Ireland too strongly shews.—I remain, my dear Sir, with much esteem, faithfully yours, J. DAVY.

BARBADOES, *Nov*. 19, 1846.

Proceedings of the Wernerian Natural History Society.

At the 40th Anniversary Meeting of this Society, held in the College on the 28th November, the following gentlemen were elected office-bearers for 1847:—Professor Jameson, President.—Dr Greville, Dr Brunton, Professor Goodsir, Dr Anderson, W. Copland, Esq., and Dr Coldstream, Vice-Presidents.—R. Stevenson, Esq., David Milne, Esq., W. A. Cadell, Esq., A. J. Adie, Esq., Dr Fleming, Professor Traill, Sir W. Jardine, and Professor Balfour, of the Council.—Dr Neill and T. J. Torrie, Esq., Joint-Secretaries; A. G. Ellis, Esq., Treasurer; J. Wilson, Esq. and Dr Hamilton, Joint-Librarians; and W. H. Townsend, Esq., Artist.

At the meeting held on the 19th December (Rev. Dr Brunton, V.P., in the chair), several very interesting communications were brought forward.

The first consisted of an account of the habits of the Equus hemionus or Wild Horse, and of the Great Indian Marmot, drawn up by Mr Laurence Jameson, from specimens received from his brother William Jameson, Esq. (Hon. E.I.C. Service, Saharunpoor), who had shot both on the plains of Chinese Tartary, neither being found on the British side of the Himalaya range. The skins of both had been sent home, and the stuffed specimens were now exhibited to the meeting.

The Equus hemionus* (as implied in the specific name) is intermediate in its characters between the horse and the ass. In comparing the wild with the domestic horse, Mr Jameson remarked, that the head is much larger in proportion to the size of the animal; the ears longer and more erect; neck finer and more slender; legs more slender; tail asinine; hoofs also asinine, and subcylindrical; mane short, and of a deep brown colour, approaching to liver brown; mouth rather small, and nostrils situate close to the muzzle; a dorsal line, of a deep chestnut-brown, extending from the bottom of the mane to two-thirds along the tail, the latter third being composed of a tuft of black hair. The general colour of the animal is characteristic: muzzle white; head, face, and external portion of ears, chestnut-brown; the body, and the external parts of the shoulders and horns, are of a wood brown colour; while the fore parts of the neck, belly, and legs are white; and the line where those two colours meet is well-defined, the colours nowhere blending into each other. Mr Jameson further mentioned that the specimen stands exactly 13 hands high; and that the other most important measurements were—length of head, 2 feet; neck, 2 feet 5 inches; back, from shoulder to rump, 4 feet 8 inches; tail, 1 foot 6 inches; legs, 4 feet 8 inches; circumference of neck, 1 foot 10 inches; girth of body, 5 feet; from flank to flank, 2 feet 6 inches; length of ears, 8 inches. The dental system was unfortunately imperfect in the specimen.

The Tartarian Marmot was found to be entirely new to natural history, and greatly superior in size to all its congeners, several American species being placed on the table. Professor Jameson has named it Arctomys tataricus, and the following is the description:—Length from nose to tip of tail, 2 feet 10 inches; length of tail, 6½ inches; girth between scapulæ or round anterior flanks, 1 foot 1 inch; girth round posterior flanks, 1 foot 5 inches; length from flank to flank, 11 inches; extremities, 8 inches; girth round throat, 11 inches; height at shoulder, 9 inches:—head and eyes rather large; ears rudimentary; nostrils black, and naked at the part which separates them; upper lip distinctly bilobated, leaving the incisors uncovered; moustaches on both sides of muzzle, and above and below the eye:—teeth, 2 incisors in the upper, and 2 also in the under jaw; 10 molars in the upper jaw, 5 on each side of the incisors, and 8 in the under jaw, or 4 on each side of the incisors, making in all 22 teeth:—the colour of the upper part of the body, and external part of extremities, yellowish-grey; face, dark grey; under part of body, inner side of legs, and lower part of throat and neck, orange-yellow:—fur of two sorts, hairy and woolly, the roots of the hairy portion being imbedded in, or surrounded by, the woolly; when a single hair is examined, it is found to be tipped with black, white in the middle, and of a slate-grey below; the fur is particularly thick on the back, flanks, belly, outer side of limbs, and especially on the cheeks, where the length of the hair changes the apparent proportions of the head.

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* Female specimen.

Dr Traill expressed, in strong terms, how much we owe to the scientific zeal and perseverance of Mr William Jameson of Saharunpoor, for the transmission to his native city of such rare and valuable specimens. He likewise made some interesting remarks on the distinction between the Hemionus and the Onager, and exhibited the figures given by Pallas and Ker Porter.

At the same meeting Mr Robert Bald, mining-engineer, gave an account of remarkable derangements occurring in the sandstone strata at Binny Craig Quarry, occasioned by trap-dykes. Specimens of the sandstone, now extensively used in building, were exhibited. It is rather homogeneous in appearance; but minute scales of mica can be observed; and it was mentioned that by noticing to keep these scales always in a horizontal position, masons could with certainty place their blocks in bed, where they remain unaltered by the weather, and not on edges when the mica is apt to permit the water to enter, and, by frost, to occasion the splitting up of the stone.

Professor Goodsir afterwards exhibited specimens alive, in sea-water, of the polyp-like young of the *Medusa aurita* (the common sea-blubber); also a group of the living *Coryne squamata*, in the act of throwing off their medusa-like larvæ. The young medusæ had been received that morning (in a well-protected phial, sent by post) from Dr Reid of St Andrews, to whose interesting researches on this subject, as well as to those of Sars and Seibold, he briefly alluded. The locomotive sea-blubbers are, at first, when young, small, fixed, polyp-like animals; whereas the fixed or rooted polyps are, when young, locomotive, and resemble minute sea-blubbers.

List of Patents granted for Scotland from 24th September to 21st December 1846.

1. To Benjamin Fothergill, of Manchester, in the county of Lancaster, machine-maker, "improvements in certain parts of machinery used in the preparation for spinning, and in spinning and doubling of cotton-wool and other fibrous substances."—24th September 1846.

2. To Joseph Gray, of Redcross Street, Southwark, machinist, "improvements in gas-meters."—24th September 1846.

3. To Robert Heath, of Heathfield, in the county of Lancaster, gentleman, "certain improvements in the construction of wheels, axles, and bearings, to be used upon rail and other roads, which improvements are also applicable to mill-gearing and other purposes."—28th September 1846.

4. To John Warburton, of Kearsley, in the county of Lancaster, tinsmith, "certain improvements in machinery or apparatus for preparing, slubbing, and roving cotton-wool and other fibrous materials."—28th September 1846.

5. To Antonio James Mayer, of Ashley, in the county of Middlesex, gentleman, "improvements in certain wood and cutting machines."—29th September 1846.

6. To Samuel Milbourn, of St Mary's Cray, in the county of Kent, paper-maker, "improvements in the manufacture of paper."—29th September 1846.

7. To Peter Fairbairn, of Leeds, in the county of York, machine-maker, and Peter Carmichael, manager for Messrs Baxter Brothers and Company, flax-spinners, Dundee, North Britain, "improvements in machinery for drawing, roving, and spinning flax, hemp, wool, silk, and other fibrous substances."—1st October 1846.

8. To John Lloyd Bullock, of Conduit Street, Hanover Square, chemist, "improvements in the manufacture of quinine."—2d October 1846.

9. To Pierre Armand Le Comte de Fontainemoreau, of 45 New Broad Street, in the city of London, English and Foreign Patent Office, being a communication from abroad, "certain improvements in producing artificial fuel."—6th October 1846.

10. To John Taylor, of the Adelphi, in the county of Middlesex, gentleman, being a communication from abroad, "improvements in the manufacture of explosive compounds."—12th October 1846.

11. To Robert Warrington, of Apothecary's Hall, in the city of London, chemist, "improvements in preserving animal and vegetable substances."—12th October 1846.

12. To William Rodger, of Shawfield Street, King's Road, Chelsea,

a Lieutenant in the Royal Navy, "improvements in anchors."—12th October 1846.

13. To John Paterson Reid, of the city of Glasgow, and county of Lanark, in North Britain, gentleman, and Thomas Johnson, of the same city, mechanic, "certain improvements in machinery for weaving, and for producing patterns and weaving, as also in machinery for finishing certain woven fabrics."—13th October 1846.

14. To John Condie, of Glasgow, engineer, "improvements in machinery used in manufacturing malleable iron."—14th October 1846.

15. To George Winslow, of Burton Crescent, in the county of Middlesex, merchant, being a communication from abroad, "improvements in machinery for manufacturing files and rasps."—19th October 1846.

16. To John Longbottom, of Edward Street, Leeds, mechanist, "improvements in the manufacture of oil-cake, and in machinery and processes for pressing and moulding the same, which machinery and processes are also applicable to the manufacturing of other articles from plastic materials."—27th October 1846.

17. To Alfred Markwick, of Langham Place, in the county of Middlesex, surgeon, being a communication from abroad, and partly an invention of his own, "improvements in the manufacture of epithems used for medical and surgical purposes."—27th October 1846.

18. To Richard Clarke Burleigh, of the city of Bath, in the county of Somerset, gentleman, "certain improvements in artificial light."—29th October 1846.

19. To James Thomas Jullion, of Stratford, in the county of Essex, analytical chemist, "improvements in the manufacture of certain acids, also improvements in decomposing certain acids, and applying the products resulting therefrom to the production of certain chemical compounds."—4th November 1846.

20. To William Church, of Birmingham, in the county of Warwick, civil-engineer, "improvements in machinery for making candlestick-pans, and other articles capable of being made wholly or in part by stamping or hammering, also in machinery for making wholly or in part lockets, or tubes for candlesticks, and tubes and tubular articles applicable to various other purposes."—4th November 1846.

21. To John M'Bride, of the firm of M'Bride and Company, cotton-spinners and power-loom cloth manufacturers, Albyn Works, Glasgow, "improvements in weaving."—11th November 1846.

22. To Joseph Douglas, of the borough and county of Newcastle-upon-Tyne, rope-maker, "improvements in the manufacture of yarn, twine, and cordage."—11th November 1846.

23. To George W. Eddy, of Waterford, in the state of New York, of the United States of America, "an improvement in the manufacture of

cast-metal wheels for railway and various other carriages."—11th November 1846.

24. To William Aitkin, of Aberdeen, in North Britain, gentleman, "certain improvements in two and four wheeled carriages."—11th November 1846.

25. To James Boydell, of Oak Farm Works, near Dudley, Stafford Inn, master, "improvements in applying apparatus to carriages to facilitate the draft."—12th November 1846.

26. To Thomas Vaux, of Frederick Street, Gray's Inn Road, in the county of Middlesex, landsurveyor, "improvements in the manufacture of horse-shoes and horse-shoe nails."—12th November 1846.

27. To George Scholefield, of Manchester, in the county of Lancaster, agent, "certain improvements in machinery or apparatus to be employed for lithographic printing."—13th November 1846.

28. To William Anderson, of Clitheroe, in the county of Lancaster, cotton-spinner, "certain improvements in machinery or apparatus for preparing and spinning cotton and other fibrous substances."—19th November 1846.

29. To Edward Alfred Cowper, of Smethwick, near Birmingham, engineer, "improvements in the manufacture of railway-chairs."—25th November 1846.

30. To William Watson Pattinson, of Felling, near Gateshead, in the county of Durham, manufacturing chemist, "improvements in the manufacture of chlorine."—25th November 1846.

31. To Charles Bertram, of Gateshead, in the county of Durham, Esq., "improvements in the manufacture of artificial fuel, and in the application of the residual products to useful purposes."—25th November 1846.

32. To Thomas Wood Gray, of Workworth Terrace, Commercial Road, in the county of Middlesex, "improvements in ports, and in apparatus for opening and closing ports of ships or vessels, also applicable in opening and closing windows, and other instruments having like movements."—27th November 1846.

33. To Alfred Vincent Newton, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, mechanical draughtsman, being a communication from abroad, "certain improvements to be applied to the grinding of grain and other substances."—28th November 1846.

34. To James Caleb Anderson, of Buttevant Castle, in the county of Cork, Ireland, Baronet, "certain improvements in obtaining motive power, and in applying it to propel carriages and vessels, and to the driving of machinery."—9th December 1846.

35. To William Mathers Hall, of Leeds, in the county of York, brassfounder, "a certain improvement, or certain improvements in and

applicable to sliding gas-pendants, lamps, lustres, and chandeliers."—9th December 1846.

36. To William Johnson, of Grosvenor Wharf, Millbank, in the city of Westminster, gentleman, "certain improvements in machinery for raising or lifting and lowering weights or ponderous bodies."—10th December 1846.

37. To William Johnson, of Grosvenor Wharf, Millbank, in the city of Westminster, gentleman, "certain improvements in propelling carriages on railways."—14th December 1846.

38. To William Eaton, of Newington, in the county of Surrey, engineer, "certain improvements in obtaining motive power."—14th December 1846.

39. To John Mercer, of Oakenshaw, in the county of Lancaster, chemist, and John Greenwood, of Church, in the same county, chemist, "certain improvements in dyeing and printing turkey-red and other colours."—16th December 1846.

40. To Bartholomew Beniowski, of Bow Street, Covent-Garden, in the county of Middlesex, Major in the late Polish Army, "certain improvements in the apparatus for and process of printing."—17th December 1846.

41. To John Todd, of Glasgow, in the county of Lanark, and William Johnston, of Birmingham, in the county of Warwick, engineer, "improvements in arranging the rails on certain parts of railways."—21st December 1846.

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PHILOSOPHICAL JOURNAL.

On the Manner in which Sound is Produced and Diffused. By Sir G. S. Mackenzie, Bart., F.R.S.; Vice-President of the Royal Society of Edinburgh, &c. Communicated by the Author.*

The science of acoustics has become so unfamiliar, that, while the comforts of the eye, the skin, the palate, and the lungs are in the thoughts of every one, those of the very important sense of hearing, in reference to music and speaking, have been thought scarcely worthy of consideration. So unused have we been to consider the philosophy of sound,—so little has our attention been turned to it of late, either in theory or in practice,—so long have architects been permitted to err in the construction of rooms intended for the distinct conveyance of sound to the ear, that we may be justified in reverting to the very rudiments of the science, and in considering, first, whether we know all that can be known in regard to the source from whence sound proceeds, or to the cause which draws it from that source.

It is a groundless expectation, that man is ever to arrive, in the progress of discovery, at the nature or essence of any thing. With respect to sound, therefore, we can do no more than attend to the immediate causes of its production, and to the laws which it obeys in its diffusion. In the present state of our knowledge concerning it, philosophers appear to have entirely rejected the idea of its being a thing *sui gene-*

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* Read before the Royal Society of Edinburgh on the 20th February 1843.

ris, as light and heat, and to have become content with the rather unsatisfactory conclusion of its being mere mechanical action. Such a conclusion, however, does not afford sufficient ground for altogether ceasing to observe the phenomena of sound, and the circumstances under which it is made known to the sense of hearing.

Motion is necessary to the evolvement of everything that we know by means of our senses; and there does not appear to be any well-defined meaning attached to the word mechanical, in reference to action. Motion being common to all action, and being regulated by force, I apprehend that the only distinction meant, in using such terms as mechanical and chemical, refers to the causes and results of action, and not to any variation in action itself. That sound is a result of mechanical action (using that term in contradistinction to action of any other kind), does not establish that it is *not* something *sui generis*. Sound is the result of chemical and electrical action also; and Sir John Herschel's definition, on that account, is confined to the result of every kind of action, viz., motion. "Sound," he says, "*consists* in a motion of the air itself, communicated along it by virtue of its elasticity." The expression, "*a* motion," is indefinite, and gives no information. It is, besides, incorrect, inasmuch as it infers the existence of more kinds of motion than one, while more than one does not exist. Motion is one; and its being capable of variation in direction, circumscription, and velocity, effects no change. The fact, however, that sound is *produced* by a motion of the air itself, communicated along it by virtue of its elasticity, is true. But sound, did it *consist* of a motion of the air itself, could not consist of anything else. Nevertheless, we may as truly say, that sound consists of a motion in hydrogen, in oxygen, in nitrogen, or in any gaseous matter, seeing that from all of them sound proceeds under similar circumstances, with more or less intensity, according to their density.*

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* Dr George Wilson recently exhibited to the Royal Scottish Society of Arts, the sounds produced by the passage of the electric spark through various gases; and the result confirmed what is stated above, and in subsequent parts of this paper. February 22, 1847.

The analogy between sound and light has been noted as remarkable, though it has not been carried out far enough; and as light and heat have been admitted to be distinct entities, and as they have distinct organs of sense (the eye and the skin), appointed to them; and as taste and smell are also made known by motion, and contact with distinct organs; and as there is a distinct organ appropriated to sound, there is no reason why it should not be regarded also as a distinct entity, though, hitherto, our observation has not been extended far enough to shew that it is such. If we once begin to discover anything hitherto unobserved respecting sound, its philosophy may again advance, and become as interesting, perhaps, as that of electricity, heat, or light.

That the mere action of one solid body on another solid body is not essential to the production of sound, is proved by the mechanical action or motion of a bell *in vacuo* producing none. The same fact proves the necessity for the presence of air; and this renders the affirmation safe, that, while it is undeniable the action of various substances in certain circumstances on each other, *produces* the *condition* necessary for the evolvement of sound, neither the action, nor the condition it brings about (motion), can be considered as the thing evolved under that condition.

The ordinary inference from the experiment of the bell *in vacuo*, is, that air is necessary for the *conduction* of sound. My inference seems equally just, that it is as necessary for the *production* of sound. The first inference implies, that sound is actually produced by the bell *in vacuo*, but is not heard on account of a conductor being absent. If air is regarded as a conductor merely, the experiment proves somewhat more, viz., that none of the materials of which the apparatus for performing the experiment consists, can be considered as *generators* of sound. If they were, sound should be heard, if produced by the bell. As no sound is heard, the inference is irresistible, that air is necessary to its *production*. This, then, is one fact decidedly established by the experiment.

The question that now occurs is, How comes air to be necessary to the production of sound? And this involves another, What is the source from which it is evolved? These questions are now put on the assumption, that motion cannot be identical with sound, as supposed in Sir J. Herschel's definition, though motion be necessary to the evolvement of sound. Sound, therefore, I consider as a thing *sui generis;* as light, heat, and electricity.*

I consider farther, *that it is contained abundantly in atmospheric air, and more or less,* in proportion to density, *in aëriform or gaseous matter;* and this, perhaps, exclusively. It can scarcely be objected, that gaseous matter cannot contain, or be combined with anything, but what is proper to its own constitution, or, more properly speaking perhaps, to its proper nature; for we know, that it owes its condition to heat; and that common air, when suddenly compressed, gives it out so abundantly as to set fire to combustible bodies. When the electric spark passes from a body in which it is concealed, through the air towards another body, light is given out. We are not, indeed, in a condition to say which gives out the light, the electric fluid or the air. Light, however, evidently exists in the one or the other, or in both. No doubt, in an exhausted receiver we see light emanating from the electric fluid dispersed through the vacuum; but the intensity is very far below that of the spark passing through the air. From this we may, at least, infer, that the air is necessary to the brilliancy of the spark. Experiments have been made to shew, that different gases have different powers of conduction in reference to sound. The results, however, in respect to my views, shew that different gases contain different quantities of sound. Other questions now suggest themselves.

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* There is sometimes a want of precision in the mode of expression made use of by some writers on sound, which contributes to the difficulty of the subject. For instance, at the outset of the article, Acoustics, in the Encyclopædia Britannica, it is said, "The report of a cannon, for example, produces a concussion which shakes the ground under, and to a great distance around it." It is evident, that, in this case, the cause of the concussion is not, properly speaking, the *report,* but the *force* elicited by the sudden liberation of highly expansive substances from confinement in the gunpowder. If a cannon could be fired in a vacuum, no *report* would be heard, but the *concussion* would be equally felt.

If, in the experiment with the bell *in vacuo*, no sound be produced, or if it be produced but not conducted by the materials of the aparatus, how is it made known that sound is produced when the air is admitted into the receiver; and how is it then conducted to the ear by the materials of the apparatus ?

Motion was produced by the bell *in vacuo*, and no sound was heard. The same motion being produced when the air was admitted, sound was heard; therefore, some effect must have been caused in the air, and some motion produced, independent of that caused by the impulse given to the bell. That this was the case, and how the sound was produced, I will endeavour presently to shew. In this, that motion was given to the air in the receiver; that the air communicated motion to the receiver; that the receiver imparted it to the air without; the air without to the drum of the ear; and the drum to that portion of the air within it, and next to the sensorium—all agree.

Instead of saying that sound *consists* of motion given to the air, I am induced by many observations to believe the cause of sound being evolved to be, in all cases, *the rushing of air into a vacuum*, the effect being a peculiar movement by which sound is evolved and conveyed. This new result of observation I will now proceed to illustrate.

That the motion of the air rushing into a vacuum, and which it does with very great velocity, does produce sound, is illustrated by the extension of an experiment usually made to exhibit the pressure of the atmosphere. A hollow vessel, having its mouth covered by a piece of bladder stretched over it, so as to have the fastening air-tight, is applied to an air-pump or exhausting-syringe. As soon as a portion of air is abstracted from the vessel, the bladder is seen to be pressed into it. Let the exhaustion be carried on as far as may be, without the bladder tearing. If an opening be now made with the point of a knife, the air rushes in, and a loud report is heard. The sound produced in this case is evidently the result of the filling up of a vacuum. It remains to be shewn that, in all cases when sound is evolved, the formation of a vacuum, more or less perfect and extensive, precedes; and its being filled, and the production of sound, are simultaneous. We shall begin with loud sounds.

When the electric fluid passes from one cloud to another, or descends to the earth, the velocity is so great, that the air is suddenly displaced along its whole passage, and a very perfect vacuum is formed. It is as suddenly supplied, and a loud crash is instantly heard. In this case, no hard bodies are interposed; it is merely the quick passage of the lightning that causes the vacuum.

When a cannon is fired, the sudden and forcible expansion of the gases which are set loose, causes them to rush out of the tube, driving the wadding, shot, and air which previously occupied it before them, and thus a vacuum is produced in the tube. The instant the expelling force ceases to act, the air violently enters to supply the vacuum, and the sound peculiar to the firing of cannon is produced.

When a string or wire is stretched between two points, and made to vibrate by its elasticity, each passage of the string from one position to another (which is rapid in proportion to the tension), displaces the air in the same manner as the electric spark, and on the filling up of the vacuum sound is produced. If there were only a single movement, there would be but a single sound; but when vibrations succeed each other at intervals so short as to be inappreciable to our sense, the formation of vacua, and their filling up, are so quick in succession, as to render it impossible for the ear to perceive any interruption, and the sound is accordingly heard, while the vibrations proceed, as prolonged or musical.

The case is the same in regard to sound produced by friction on strings, an extremely rapid succession and filling of vacua being the result of the very minute starts of the bow in overcoming the friction.

When a blow is struck with any substance on another, with a hammer, for instance, on a resisting body, the force of the blow, at the moment of contact, displaces the air at and around the spot, so as to form a vacuum, and the air instantly recovering its position, sound is evolved.

In writing, a scratching noise is heard. This is caused by the pen jerking over the rugosities of the paper, causing minute vacua, which are instantaneously filled.

With respect to soft or low sounds, it must be premised, that a body in motion, when surrounded by a medium, necessarily displaces that medium in its progress. A vacuum is the effect of the removal of the body from its position; and in proportion to the velocity of removal, is the extent of the vacuum left at any one instant of time. Sound is modified in its amount by the bulk of air displaced, and in its intensity by the velocity of displacement. The latter is regulated by elasticity; for example, the sound produced by the collision of two elastic bodies is far more intense than when two inelastic bodies come into contact, the velocity being the same in both cases. The faster we write, the louder is the sound of scratching; and we may write so slow that no sound is produced that is sensible to the ear. Writing with the comparatively inelastic pencil, the sound is far less intense than when the elastic pen is used.

When a body is moved slowly through the air, or whe the air slowly passes a body at rest, which is the same thing in effect, the vacuum at any one instant is small, and is every instant supplied, and no sensible sound follows. When a body is moved rapidly, a more extensive vacuum is every instant formed, the collapse is proportional, and sound is heard.

A farther illustration may be found in the case of the cannon. When a full charge is put into a cannon, the spaces for the gases set free by the firing of the powder is confined, and the resistance to their expansion causes a high degree of velocity in their escape; and the rush of air to supply their place in the tube, causes the sound precisely in the same manner as it does in the case of the exhausted vessel covered by bladder, already mentioned. When a charge sufficient only for a musket is put into a cannon, there is more room for expansion, in proportion to the quantity of gas evolved; the velocity of expulsion is less, and the sound produced is feeble. But if the small charge be put into a musket, there is a proportional resistance to the expansive force, and a proportional intensity of sound. The amount, or mass, of sound evolved in the case of circumscribed portions of air, depends, *cæteris paribus*, on the extent of the vacuum produced. This is illustrated by the pipes in an organ. The velocity of the air passing into the pipes, is regulated by the weight on the bellows, so that the pressure is uniform for all the pipes, from the smallest to the largest. The size of the pipe, and width of the languid, regulates the *amount* of sound, on which gravity or acuteness depend, the pressure being the same. The *intensity* depends on the force or velocity of the air passing into the pipe. The pressure being uniform, when it is exerted on a large pipe, brings out a grave sound; and when exerted on a small pipe—on a small quantity of air—the sound is more intense or acute. In the former case, the mass of sound being greater, though not so intense, is heard at a greater distance than that produced by the small pipes. Accordingly, we hear the sound produced by the pedal pipes of an organ, on the outside of a church, when we can distinguish no other. Supposing that all the pipes were of the same size, and each to have its own bellows, if the force applied to expel the air from all the bellows were as 1, the same sound would be produced from all the pipes. But suppose the force to be augmented as 2, 3, and so on, in different bellows, the sound would be proportionally intense or acute. This is illustrated also by the flute, in which a note may be raised an octave by blowing harder on the same hole. The greater the force exerted on the same amount of air, the more perfect is the vacuum, and more intense or acute the sound. The force being diminished, the sound becomes more grave. The sound from organ pipes is brought out by forcing air over a serrated surface (the languid), which, by causing a number of rapid movements, produces the necessary condition. In reed pipes, the vibration of a piece of metal produce the vacua. The sound of wooden pipes in the organ is modified by what is called stopping them—closing them with a plug. By this means, a short pipe is made to give the same note as a long one, but softer. The cause of this is, that the air forced into the pipe is resisted and sent back, thus diminishing the rate of motion in the air, and the amount and intensity of the sound.

The facts which I have stated appear sufficient to establish that, in all cases in which sound is produced, a vacuum is formed and filled. This, indeed, must be the case whereever there is motion, such as to cause a body to change its place, when surrounded by an elastic medium, such as air. I, therefore, now proceed to consider in what manner sound is made known to us.

Sound is said to travel through the air at the rate of 1130 feet in a second of time; and *that* whether it goes to a long or a short distance. Now, if, according to Sir John Herschel, sound *consists* of *motion*, we may say that, *a* motion (but not such a motion as one that causes advance), being communicated to the air, it is somehow propagated at the above rate. But *rate* is proportional to *impulse*, and that of sound does not vary with that of impulse. Hence it may be argued that sound, when evolved, moves independently, as light does; and that, though it has motion, it is something different from motion. A mass of air set in motion produces no sound until it meets resistance; not the most violent hurricane. The motion of the air which produces sound, is not the advancing motion of a mass, but one given to it, and assumed, and somehow propagated, as Sir J. Herschel says, in virtue of its elasticity. This peculiar motion among its parts, is evidently the cause of the conveyance or propagation of sound. But how its *rate* should be uniform, while the impulses that cause it vary, is a question unanswered hitherto; unless sound be analogous to light in respect to its uniform rate, being something *sui generis*. It is unnecessary to repeat how the motion exciting the perception of sound, is propagated through the air, as it is described in every treatise on acoustics. The *rate* of ordinary motion through various substances, has not yet been ascertained; but, probably, it depends on degrees of density and elasticity, both in the body giving and in that receiving impulse. Whatever be the impulse or degree of force producing the peculiar motion by which sound is made known, the *rate* through the air, as before noticed, is uniform; for sound, whether loud or soft, though it does not go to a uniform distance, travels at a uniform rate. It will be interesting to ascertain whether motion communicated to solids follows a similar law in passing through them, The experiments of Biot have been supposed conclusive in regard to the rate of the passage of sound through the substance of cast-iron pipes, in comparison with the rate of transmission through the internal column of air; sound appearing to have passed through the iron ten times faster than through the included air. Now, in this case, the condition of the included column, being very different from that of the unconfined air, we have no comparison between the rate in the iron and the rate in the open air, which it is of importance to know. It is obvious, that considerable retardation of the motion of the air in a tube is effected by the rough surface of the iron, and by the numerous reflections it has to undergo. It might have been predicted that, owing to such causes, some difference would be observed. In respect to the rate of motion through solids, whether it be uniform under every degree of impulse, as in the case of sound and air, we must leave it for future investigation, when proper opportunities present themselves.

If we assume that sound is something, *sui generis*, brought out of air by a *peculiar* motion imparted to it by the collapse of air filling a vacuum, though we cannot define what the peculiarity is, we may trace the manner in which it affects the sensorium. Let us then conceive the peculiar motion referred to, to be propagated in the manner of pulses or undulations, as described by writers on acoustics. First, there is a vacuum formed, and the peculiar concussion on its being filled is given, and sound produced. The concussive motion is communicated to the next portion of air, and by its means another vacuum is formed and filled, and a second concussion takes place. This is communicated to a third portion of air, and so on with great rapidity, in virtue of the elasticity of the air, till an impulse is given to the tympanum, and by it transmitted to the air contained in the internal ear. The structure of the tympanum gives it power to regulate the force of concussional impressions, so as not to permit too great force to be exerted on the nervous expansion within, which communicates to the sensorium the sound brought out of the confined portion of air. How this is effected we do not know, any more than how the internal apparatus, connected with the other senses, act in conveying impressions to the mind.

This theory, derived from the intervention of the tympanum, implies that what we hear, is not the actual sound evolved at the spot where the first collapse takes place, when we are at a distance from it, but only that which is produced from the air in the internal ear, when it has received impulse. This last is modified by external influence, so that the perceptions of distance, intensity, and quality, are conveyed. If any difficulty should appear to rest on this part of the subject, it will be found equally to rest on the common theory.

With reference to the transmission of sound through solids, I do not suppose there is any such thing; but that motion only, or perhaps more strictly speaking, impulse, is transmitted. When a blow is struck on the end of a long mass of iron at rest, air is displaced, the vacuum filled, and sound produced at the spot where the blow is given. The impulse of the blow is transmitted to the other end, and there affects the air, communicating impulse, and thus causing sound to be produced. In the case of bodies of little density, such as a log of wood, there being air in its cells and tubes, sound is produced as the impulse advances; but the sound heard when the ear is placed in contact, or nearly in contact, with the farther end, is only the sound produced by the transmitted impulse on the internal ear, which requires but little force to effect the nervous expansion. The cause of the motion of the air preserving its intensity in its passage through a tube, is simply that radiation and dispersion are prevented to a great extent; and retardation by reflections is greatly reduced by smoothness and elasticity of the interior surface. When air, in the condition of giving out sound, impinges on an inelastic surface, the motion is not reflected at all. An organ pipe lined with flannel gives no sound.

There are many facts connected with sound which remain to be investigated, in reference to the mode of its production, and the influence upon it of the forms and substances of musical instruments. These seem to confirm the notion of sound being something *sui generis;* but, at present, it would be premature to discuss them. In the mean time, the fact of its being necessary that a vacuum should be formed and filled before sound is evolved, is stated for the purpose of inducing others to pursue the subject.

On the Soil of Palestine—its Deterioration—its Productions—Food of the Orientals—Fertility of Palestine—Means of Restoration—Return of the Jews—Peculiar features of the Landscape—Inhabitants—Mahomedan Civilization—Depopulation.

We had now taken our leave of the Holy Land, and had entered the narrow domain of the ancient Phœnice. The boundary of the two states was hardly more than five miles distant from Tyre. In passing from Idumea to Jerusalem, and thence to the Sea of Galilee and the shore of the Mediterranean, we had literally gone through "the length and breadth of the land." We had seen the sites and remains of the most famous Jewish cities, and traversed those portions of country which were formerly most fruitful and populous. It was a matter of deep regret with me, that circumstances did not allow me to extend my excursions to the regions beyond the Jordan, the inheritance of the two and a half tribes—and more extensively along the shores of the Mediterranean. Still, I had seen much of the ancient Palestine, and my tour, so far as that interesting region is concerned, had now terminated. I avail myself of the occasion to introduce some general observations upon the country, for which I have not hitherto found an appropriate place.

The soil of Palestine was the subject on which, of all others connected with the country, I found that my information was most defective. The statements which I had seen were contradictory and irreconcilable. One class of writers represent the country as barren as well as desolate, and use the fact, either as an argument against the credibility of the Bible, which ascribes to this soil the greatest fertility, or as a clear demonstration of the divine origin of the Bible, which has so many predictions of the utter ruin that has fallen upon the country no less than upon its guilty inhabitants. To those who are unable to perceive in the prophetic books any clear proof that the soil of Palestine has been specially doomed to a miraculous sterility, it is difficult to believe that a region once so famed for its exuberant fruitfulness should have fallen into a degree of barrenness which returning civilization and industry might not easily remedy. The other class of writers appear chiefly concerned to remove or palliate this difficulty; and while they depict the existing evidences of the natural fertility of the soil in terms that often awaken some suspicion of exaggeration, they are ever making large allowance for the observable and wide-spread effects of the prophetic malediction. Perhaps I describe the mistaken views of ill-informed readers rather than the sentiments of any considerable number of respectable travellers and writers who have treated of this country. I have at least expressed the impressions derived from my reading upon the subject, and which I carried with me to the Holy Land.

It is quite certain, I think, that some portions of Palestine, once fertile, are now irreclaimable. The entire destruction of the wood that formerly covered the mountains, and the utter neglect of the terraces, which supported the soil upon steep declivities, have given full scope to the rains, which have left many tracts of bare rock, where formerly were vineyards and corn-fields. It is likely, too, that the disappearance of trees from the higher grounds, where they invited and arrested the passing clouds, may have diminished the quantity of rain, and so have exposed the whole country, in a greater degree, to the evils of drought, and doomed some particular tracts to absolute sterility. Besides these, I do not recognise any permanent and invincible causes of barrenness, or any physical obstacles in the way of restoring this fine country to its pristine fertility. These causes are not peculiar to Palestine. They exist, perhaps to a still greater extent, in Greece and the islands of the Archipelago, and in the mountainous regions of Asia Minor. The soil of the whole country has certainly deteriorated, under bad husbandry and the entire neglect of the means of improvement. But a small degree of skill and industry would generally be sufficient to reclaim it, as must be evident to every traveller who has observed the vineyards near Hebron and Bethlehem, and the gardens of Nablous. The region about Jerusalem, and east towards the Dead Sea and the Jordan, is the worst by far which I saw in Palestine. The mountain tract traversed by the road to Jericho, and adjacent to that route on the north, is steril, and, at least to a great extent, was always so. Still, the rocky soil of the "hill country," which extends from beyond Hebron on the south to some distance north of Jerusalem, and formerly included in the inheritances of Judah and Benjamin, is very susceptible of being restored to profitable cultivation. The innumerable remains of terraces and cisterns, and the ruins of large towns and villages thickly scattered over this romantic region, would clearly demonstrate, even if both sacred and profane history were silent upon the subject, that it has been densely peopled and highly cultivated. By far the largest portion of this mountain tract is susceptible of being fully restored to its ancient fertility. The valleys produce wheat very well at present, and the tops of the mountains, though utterly neglected, are covered with fine pasturage.

But this is the proper region for the olive and the vine. Anciently these hills were covered with orchards of fruit-trees and vineyards, and the world does not probably produce finer grapes, figs, and olives, than are annually gathered about Hebron and Bethlehem. One acre of the flinty surface of the Mount of Olives, carefully tended in olive-trees, would yield more, through the exchanges of commerce, towards human subsistence than a much larger tract of the richest Ohio bottoms tilled in corn. Most persons know little of the variety and importance of the uses to which the fruit of the olive is applied in the eastern nations, and in some of the southern countries of Europe. Large quantities of the berries are used by the inhabitants and exported as food; but the principal value of the olive consists in the delicious oil that is extracted from its fruit. This is used upon the table, and in cookery, as the substitute for both butter and lard. It is universally burned in lamps, and instead of candles, which are nearly unknown in the east. It is the principal material employed in making soap, and it is exclusively used in lubricating machinery in all the great manufacturing establishments in the world. The products of the vine and fig tree, besides entering largely into the food of the people, become also the basis of trade in a variety of forms, preserved and manufactured. To an American, whose diet is composed of bread, animal food, and vegetables, it seldom occurs to think of fruits, however delicious and abundant, as common articles of food; and the "oil and wine," the fig-trees and pomegranates of the Promised Land, are little intelligible, excepts as terms typical of general fruitfulness and fertility. They were to the Israelites, however, as they still are to the people of many parts of the east, what our corn and wheat fields are to us, and as the basis of commerce, what our tobacco, rice, and cotton fields are. The "hill country" of Judah, now the worst part of Palestine, was precisely adapted, in soil and climate, to the growth of these important staples, and they made it, perhaps, the most wealthy and populous part of the land. In those portions of the south of France adapted to the olive and vine, the proprietors of the soil cannot afford to devote it to any other species of culture; and I often heard it said there, that if the vineyards and oliveyards yielded a fair crop only once in two or three years, it was still the most profitable species of agriculture in which they could engage. In one wealthy and populous department, not ill adapted to the growth of wheat, it is estimated that only enough grain is grown to supply the inhabitants six weeks in the year. The rest is imported from other parts of the country, where these precious fruits do not attain to such perfection. I can see no reason why the replanting of the fruit trees and vineyards of the land of Judah might not enable it to support as large a population as it did in the days of Herod or of David.

A few hours south of Jerusalem there is an evident improvement in the soil, and the traveller begins to realize that he is in a good land, a land of brooks of water, of fountains and depths, that spring out of the valleys and hills. The "mountains of Ephraim" are at this day the best cultivated part of Palestine, a peculiarity to which their security from the Bedouins contributes, perhaps, more than the natural advantages of the soil. Still the land is fertile. Wherever wheat was sown, in the valleys or on the loftiest terraces, it looked well, especially when the utter want of skilful tillage, and the inattention to manuring, which universally prevail, are taken into the account. The vines, and the fig, olive, pomegranate, and other fruit trees, had universally a thrifty and even luxuriant appearance. They seemed to thrive best in the most unpromising places; and wherever a break in the rock would allow of the planting of an olive or fig tree, it appeared to attain its utmost size and perfection. I was often reminded of the doubtful Scripture phrase, "oil out of the flinty rock." I ought to say that there is no waste land in this country. Wherever there is soil, it is productive. The slight exceptions to this remark exist on the edge of the desert, and on the sea-shore. I mean to say, there is no untillable swamp or marsh-land—no tracts too wet, or cold, or poor, to promise fair returns to the husbandmen. I need not add to what has already been said of the fertility of the great plain of Esdraelon, which, under various names, and with some intervening mountain ridges, stretches from the Sea of Galilee to the neighbourhood of Acre and the Mediterranean. Almost every part of Palestine seems very capable of producing bread for its inhabitants, but this is by eminence the corn country of the Holy Land, and, under proper tillage, would supply bread stuff for millions. Palestine, we know, exported corn in the time of Solomon, when at the height of its population, and also in the age of Herod, when, too, it was fully peopled.

I put this question to nearly every traveller whom I met within or from the Holy Land, What is your opinion of the natural fertility of this country, and of its capability to feed a large population? and in every instance the reply was strongly in corroboration of the sentiments I have here advanced.

Any practical attempt to restore Palestine to its former prosperity must be based on the revival of agriculture. There is now really no basis for any extension of commerce, and a colony of tradesmen, such, for instance, as the return of the Jews would give the country, would only increase its poverty and wretchedness. The Duke of Ragusa advised Mahommed Ali to make Palestine and Syria an immense sheep-walk, and this should probably be the first object of a colony here. Every part of the country is adapted to this business, and wool to a vast amount might be raised for exportation, with little expense beyond that of multiplying the race of sheep and shearing their fleeces. The average price of sheep is now seventy piasters, nearly four dollars, though they live well throughout the year upon the natural pastures. Large additional tracts might also be tilled in wheat, with no greater labour of preparation and improvement than that of turning up the soil with a plough. The product of cotton and tobacco, which are already articles of export, might be doubled or trebled at once.

Plantations of vines, olive, fig, and other fruit-bearing trees would require more time, and return slower profits; but they would be indispensable to the complete renovation of the country, and the full development of its resources. All this appears easy enough; and American or English farmers would certainly find their account in emigrating to the plains of Esdraelon rather than to the prairies of Illinois, but for political objections. There is here no security for property. The government is itself the grand robber, in whose presence enterprise and industry cannot exist. No changes which have occurred, or can occur, in the supreme authority will remove, though they may slightly mitigate, this constitutional and incurable vice of all Oriental Governments. European merchants could not live in the east, except under the protection of their own consuls. They never become subjects to the native rulers. If some civilized Christian power would rescue Palestine, by treaty or force, from Mahomedan rule, and establish an enlightened, equitable, and stable government, then might it become a desirable residence for civilized men; but on no other condition could a residence there be endured by any but barbarians, content to be poor, and tolerant from long habit of oppression and injustice. It fills me with surprise to see some of the best men of England labouring to promote the colonization of Jews in Palestine, and that under existing governments. The Jews are precisely the last people on earth fitted for such an enterprise, as they are a nation of traffickers, and know nothing of agriculture. Besides, the Jews of Europe and America are civilized and wealthy, and would not relish oppression and robbery, even in Palestine. It would be worth while, also, for the Christian philanthropist to inquire whether the probabilities in favour of their conversion to Christianity would be multiplied by this restoration to their father land, where a thousand circumstances would perpetually remind them of the glorious days of their nation and its religion. Thus far the experiment which is now in progress upon a diminutive scale affords little encouragement to more enlarged enterprise.

Some features, common to Palestine with several other eastern countries, have been noticed incidentally in the foregoing pages, a consideration of which will aid the reader in forming a just conception of the general appearance and condition of the Holy Land. One striking peculiarity is its destitution of timber. With the slight exceptions upon Mount Tabor and the neighbouring hills, and those so lately mentioned between Safet and Tyre, I saw no native wood in Palestine. As a general remark, the entire country is bare, and the eye wanders to the boundaries of the visible horizon. The traveller prosecutes his journey, day after day, without seeing a single forest tree, with the exception, perhaps, of a stunted thorn, less than a dozen of feet in height, or a few diminutive prickly oaks, not more than two or three inches in diameter.

Another remarkable feature, which is universal, and only less striking than the former in its influence upon the landscape, is the entire want of enclosures in the agricultural districts. There are neither fences, walls, nor hedges, nor any substitute for them, the whole country being one immense common. The only exception is found in a few enclosed gardens and vineyards close to the walls of some of the towns. The limits of a field are usually marked by a narrow strip of unploughed ground—sometimes by a rough pillar or heap of stones. The crops are secured against the cattle only by the watchful care of the herdsman, who usually keeps them at a distance upon the hills. The peasant is liable to perpetual injury from this quarter. Our muleteers never hesitated to ride into a field of wheat, and graze their animals upon the growing or ripening harvest; and so universal is this abuse, and so little respected are the rights of property, that the peasants do not even remonstrate, but look in silence upon the wasting of the fruit of their labours.

I have lately had occasion to notice the rare instance of a solitary rural cottage, which I passed near the fortress of Tibinin. I do not remember another such instance in Palestine. The people congregate in villages, seated, usually, upon some point of the mountain or hill that overlooks the valley, where they perform their daily labours. There are no barns or other buildings in the plains. The harvest is thrashed upon the field by the treading of oxen or horses, and the grain carried home to the village, or to the market. The effect of these gregarious habits to increase the general aspect of desolateness and waste may be readily imagined.

Another peculiarity of great importance and effect in giving to the landscape its dreary and special character, is the want of roads, of which there are none in Palestine. Travel and transport being all performed on the backs of beasts of burden, which usually move in single file, the most important routes are only marked by narrow winding paths, that receive their direction from the ever varying features of the region over which they pass. The soil is often so hard as to take no impression from the feet of animals, and the eye of an unpractised traveller there perceives, even upon a common thoroughfare, no evidence that others have passed along the same way. No repairs are ever made—no labour employed to remove an obstacle or prevent a breach. If a rock rolls down from the mountain, or a chasm is made by a sweeping torrent, the next passer goes around it the best way he can, and henceforth there is, to the needful extent, a change in the road. Where the want of room to deviate from the old path, or any other cause, has for a long time confined the travel to a single track, a narrow channel is usually worn deep into the earth, or even into the face of the rock, which is ordinarily paved, or half filled up with rolling stones, thus forming the worst possible foot hold, and the most uncomfortable accommodation to the feet of the patient horse or donkey.

The great road leading across the plain from the Damascus gate, at Jerusalem, has been worn by travel and washed by rains till it has the appearance of a deep artificial trench, into which an infinite number of small rolling stones have been gathered from the adjacent fields. It is hardly necessary to remark that, where there are no roads there can be no wheel carriages. I did not see so much as a cart or a wheelbarrow in the Holy Land. Solomon and some other kings had carriages, and the invaders of the land often increased the terror of their approach by war-chariots. It is evident, however, that the royal equipages must have been confined to a few short routes, and these ill-adapted instruments of war could never have left the plains; The hill country was as unfavourable as possible to their movements, and it is sufficiently evident that convenient artificial roads never existed here to any great extent, with the exception of the few military routes constructed by the Romans during their sway over these countries.

Every reader will appreciate the influence of the circumstances here enumerated upon the general appearance of Palestine, and perceive that, altogether, they must modify the face of the entire region into a landscape, as unlike as possible to any thing we are accustomed to look upon in this western hemisphere. The contrast will be still heightened, and rendered more perfect if he will be at the pains of recollecting some of the no less striking peculiarities that attach to the living features of the picture; that the people wear neither hats, bonnets, nor stockings; that both sexes appear in loose, flowing dresses, and red or yellow slippers; the men wear red caps, with or without turbans; the women are concealed by white veils, with the exception of the eyes.

It has not fallen within my plan to allow much space to observations upon the character and condition of the present inhabitants of Palestine; I have, indeed, enjoyed fewer opportunities for becoming acquainted with the rural population than I could have wished, and than I expected. My chief attention was due to the natural features of the country, and to the remains of antiquity still existing upon its surface—objects which, from their manifold and profoundly interesting relation to sacred history, must ever attract the principal regard of intelligent and Christian travellers in the Holy Land. My illness on the termination of my journey through the desert, brief as it was, left me little time for less important pursuits and inquiries.

The inhabitants of Palestine are Arabs; that is, they speak the

Arabic, though, with slight exceptions, they are probably all descendants of the old inhabitants of Syria. They are a fine spirited race of men, and have given Mohammed Ali much trouble in subduing them, and still more in retaining them in subjection. They are said to be industrious for orientals, and to have the right elements for becoming, under better auspices, a civilized intellectual nation. I believe, however, it will be found impracticable to raise any people to a respectable, social, and moral state under a Turkish or Egyptian, or any other Mahomedan government. The inherent vices of the religious system enter, and, from their unavoidable connections, must enter so deeply into the political administration, that any reform in government or improvement in the people beyond temporary alleviations of evils too pressing to be endured, cannot reasonably be expected.

The Turks and Syrians are about at the maximum of the civilization possible to Mahomedans of the present time. The mercantile class is said to be little respected, and generally to lack integrity. Veracity is held very lightly by all classes. The people are commonly temperate and frugal, which may be denominated oriental virtues. Their situation, with regard to the physical means of comfort and subsistence, are, in many respects, favourable, and under a tolerable government would be almost unequalled. As it is, the Syrian peasant and his family fare much better than the labouring classes of Europe. The mildness of the climate, the abundance of land and its fertility, with the free and luxuriant pasturage that covers the mountains and the plains, render it nearly impossible that the peasant should not be well supplied with bread, fruit, meat, and milk. I have already had occasion to repeat the declaration that this people almost always appear well clothed. Their houses, too, though often of a slight construction and mean appearance, must be pronounced commodious, when compared with the dark, crowded apartments usually occupied by the corresponding classes in Europe. Agricultural wages vary a good deal in different parts of the country; but I had reason to conclude that the average was not less than three or four piasters per day. With all these advantages, population is on the decline; so active and destructive are the vicious tendencies of the reigning system of religion and government. Polygamy, military conscription, unequal and oppressive taxation, forced labour for the rulers, general insecurity of property, and the consequent discouragement of industry, are probably the principal causes of this deplorable result. There are other causes of depopulation which are inseparable from general ignorance and barbarism. One of the most destructive, and, at the same time, most latent, is probably the want of medical knowledge and skill. There are no well taught physicians; and in the hands of the ignorant pretenders, who always thrive under such circumstances, diseases come armed with a fatal malignity unknown in civilized countries. The plague often sweeps, unchecked, over the country as well as the towns, carrying off a tenth, a fifth, or a third of the inhabitants. The more common and milder diseases, which readily submit to proper treatment, often acquire the greatest virulence, through neglect and mismanagement, till they yield only to the great destroyer in the extent of their ravages.

The unskilfulness of all agricultural operations is not entitled to a place among these causes of decay in an ample and fertile region, where the most imperfect tillage can hardly fail to supply the wants of so scanty a population, yet the mere substitution of the plough of Palestine for the improved instrument used in England and Belgium would diminish the productions of the earth in those countries one-third in amount, and overspread the land with poverty and famine.—*Olin's Travels in Egypt, Arabia Petræa, and the Holy Land*, vol. ii., p. 427.

Teleology of the Skeleton of Fishes. By Professor OWEN.

To determine the parts of the vertebrate skeleton which are most constant; to trace their general, serial, and special homologies, under all the various modifications by which they are adapted to the several modes and spheres, and grades of existence of the different species, should be the great aim of osteological science; as being that which will reduce its facts to the most natural order, and their exposition to the simplest expressions. It is impossible, in pursuing the requisite comparisons upwards through the higher organized classes, not to recognize the close and interesting analogies between the mature states and forms of ichthyic organs, and the embryonic condition of the same parts, in the higher species. But these analogies have been frequently overstated, or presented under unqualified metaphorical expressions, calculated to mislead the student, and to obstruct the attainment of true conceptions of their nature. We should lose some most valuable fruits of anatomical study were we to limit the application of its facts to the elucidation of the unity of the vertebrate type of organization, or if we were to rest satisfied with the detection of the analogies between the embryos of higher and the adults of lower species in the scale of being. We must go further, and in a different direction, to gain a view of the beautiful and fruitful physiological principle of the relation of each adaptation to its appropriate function, and if we would avoid the danger of mistaken analogy for homology or identity, and of attributing to inadequate hypothetical secondary causes the manifestations of Design, of Supreme Wisdom and Beneficence, which the various forms of the animal creation offer to our contemplation.

To revert, then, to the skeleton of Fishes, with a view to the teleological application of the facts determined by study of this complex modification of the animal framework. No doubt there is analogy between the cartilaginous state of the endo-skeleton of Cuvier's Chondropterygians, and that of the same part in the embryos of air-breathing vertebrates; but why this gristly skeleton should be, as it commonly has been pronounced to be, absolutely inferior to the bony one, is not so obvious. The ordinary course of age and decrepitude, or of what may be called the decay of the living body, is associated with a progressive accumulation of earthy and inorganic particles, gradually impeding and stiffening the movements, and finally stopping the play of the vital machine; and I know not why a flexible vascular animal substance should be supposed to be raised in the histological scale because it has become impregnated, and, as it were, petrified, by the abundant intus-susception of earthy salts in its areolar tissue. It is perfectly intelligible that this accelerated progress to the inorganic state may be requisite for some special office of such calcified parts in the individual economy; but not, therefore, that it is in an absolute elevation of such parts in the series of animal tissues.

It has been deemed no mean result of comparative anatomy to have pointed out the analogy between the shark's skeleton and that of the human embryo, in their histological conditions; and, no doubt, it is a very interesting one. But can no insight be gained into the purpose of the all-wise Creator, in so arresting the ordinary course of osteogeny in the highly organized fish? Are we to entertain no other view of it than as an unfinished, incomplete, stage of an hypothetical serial development of organic forms.

The predaceous sharks are the most active and vigorous of fishes; like the birds of prey, they soar, as it were, in the upper regions of their atmosphere, and, without any aid from a modified respiratory apparatus, devoid of an air-bladder, they habitually maintain themselves near the surface of the sea, by the actions of their large and muscular fins. The gristly skeleton is in prospective harmony with this mode and sphere of life, and we shall subsequently find as well marked modifications of the digestive and other systems of the shark, by which the body is rendered as light, and the space which encroaches on the muscular system as small, as might be compatible with those actions. Besides, lightness, toughness, and elasticity, are the qualities of the skeleton most essential to the shark; to yield to the contraction of the lateral inflectors, and aid in the recoil, are the functions which the spine is mainly required to fulfil in the act of locomotion, and to which its alternating elastic balls of fluid, and semi-ossified bi-concave vertebræ, so admirably adapt it. To have had their entire skeleton consolidated, and loaded with earthy matter, would have been an encumbrance, altogether at variance with the offices which the sharks are appointed to fulfil in the economy of the great deep.

Yet there are some who would shut out, by easily comprehended but quite gratuitous systems of progressive transmutation and selfcreative forces, the soul-expanding appreciations of the final purposes of the fecund varieties of the animal structures by which we are drawn nearer to the Great First Cause. They see nothing more in this modification of the skeleton, which is so beautifully adapted to the exigencies of the highest organized of fishes, than a foreshewing of the cartilaginous condition of the reptilian embryo in an enormous tadpole, arrested at an incomplete stage of typical development. But they have been deceived by the common name given to the Plagiostomous fishes; the animal basis of the shark's skeleton is not cartilage; it is not that consolidated jelly which forms the basis of the bones of higher vertebrates; it has more resemblance to mucus; it requires 1000 times its weight of boiling water for its solution, and is neither precipitated by infusion of galls, nor yields any gelatine upon evaporation.

In like manner, the modifications of the dermal skeleton of fishes have been viewed too exclusively in a retrospective relation with the prevalent character of the skeleton of the invertebrate animals. Doubtless it is in the lowest class of vertebrata that the examples of great and exclusive development of the exo-skeleton are most numerous; but some anatomists, in their zeal to trace the serial progression of animal forms, seem to have lost sight of all the vertebrate instances of the bony dermal skeleton, except those presented by the ganoid and placoid fishes. He must have sunk to the low conception, that nature had been limited to a certain allowance of the salts of lime in the formation of each animal's skeleton, who could affirm, that in the higher vertebrata "the internal articulated skeleton takes all the earthy matter for its consolidation (xxvii., p. 537), forgetting that the bulky Glyptodon, and its diminutive the Armadillos, have their internal skeleton as fully developed, and as completely ossified, as in any other mammals. The organizing energies which perfect and strengthen the osseous internal skeleton do not destroy, nor in any degree diminish the tendency to calcareous depositions on the surface, when the habits and sphere of life of the warm-blooded quadruped require a strong defensive covering from that source.

The moment that the observations of the naturalist bring to light the mode of life of any of those fishes which are said to retain an unusual proportion of the external shell of the invertebrata, we are in a condition to appreciate the adaptation of that external defensive covering to such mode of life. The sturgeons, for example, were designed to be the scavengers of the great rivers; they swim low, grovel along the bottom, feeding in shoals, on the decomposing animal and vegetable substances which are hurried down with the debris of the continents drained by those rapid currents: thus they are ever busied re-converting the substances which, otherwise, would tend to corrupt the ocean, into living organized matter. These fishes are, therefore, duly weighed by a ballast of dense dermal osseous plates, not scattered at random over their surface, but regularly arranged, as the seaman knows how ballast should be, in orderly series along the middle and at the sides of the body. The protection against the water-logged timber and stones hurried along their feeding grounds which the sturgeons derive from their scale-armour, renders needless the ossification of the cartilaginous case of the brain or other parts of the endo-skeleton to be kept as light as may be compatible with its elastic property and other functions. The sturgeons are further adjusted to their place in the liquid element, and endowed with the power of changing their level, and rising with their defensive load to the surface by a large expansive air-bladder.

These teleological interpretations of the dermal bony plates may give some insight into the habits and conditions of existence of those ganoid and heavily protected placoid fishes which so predominated in the earlier periods of animal life in our planet; whereas these ganoids and placoids have hitherto been viewed almost exclusively by the light of the analogy of an embryonic "Age of Fishes," or explained by the hypothesis of transmuted crustacea. Some have gone so far as to affirm, that in all those solid parts that cover and shield the exterior of the body of the sturgeon and analogous fishes, "there is nothing in the least analogous to any part of the internal articulated skeleton of vertebrata," but that "it is entirely a remnant of the superficial shells of invertebrata," (xxvii., p. 337.) You would hardly suppose, from these exaggerated expressions, that both ganoid and placoid plates are as richly organized and permeated by nutrient vessels as the bones within; and that they present the same microscopic structure as the ossified parts of the endo-skeleton, which they serve to protect. I have proved this with regard to the existing *Lepidostens*, and the extinct *Lepidotus* (v., p. 14). Drs Peters and Müller have shewn the osseous rayed corpuscles in the scales of polypterus and other ganoids. Nay, many of the Ganoid fishes have these modified bony scales articulated in regular series by a kind of gomphosis, like the pegs and sockets by which the tiles of a roof are linked together. The dermal bones which form the carapace of the armadillo have the same cellulo-reticulate interior structure as the carpal, tarsal, or other bones of the endo-skeleton not excavated by a medullary cavity. This is well demonstrated in the dermal bones of the great extinct Glyptodons.*

The great proportion of the primitive cartilage which is retained in the skull of many of the osseous fishes, the salmon and pike, for example, and the greater proportion of the animal to the earthy matter in all the bones, their coarse texture, the radiating fibres of the flat cranial bones, and the general absence of dentated sutures, are all persistent characters in osseous fishes, which remind the anthropotomist of transitional ones in the human fœtus; but the light of teleology demonstrates the perfection of such, so termed embryonic conditions, in relation to the atmosphere and movements of the fish. It is generally in fresh-water abdominal fishes that the semi-osseous condition of the skull is found, and the diminution of the quantity of heavy earthy particles may be connected with the less dense quality of their medium, as compared with sea-water, and with the usually more posterior position of the ventral fins.

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* M. De Blainville, in the "Généralités Ostéologiques," prefixed to his great "Osteographie," admits that the structure of the dermal bones has a certain resemblance with that of true bones; but errs in stating, "avec cette difference importante, qu'elle n' est jamais celluleuse et reticulée," 4to 1839, p. 12.

In reference to the analogies to the form of a fish, we may be reminded that the head of the human embryo is disproportionately large. True, but the head of a fish must needs be large to meet and overcome the resistance of the fluid, in the mode most favourable for rapid progression; it must, therefore, grow with the growth of the fish. Hence, the large cranial bones always shew the radiating osseous spiculæ in their clear circumference, which is the active seat of growth; hence, the number of overlapping sutures, which least oppose the progressive extension of the bones. The cranial cavity expands with the expansion of the head, the absorbents remove from within as the arteries are extending the osseous walls without, but the brain undergoes no corresponding increase; it lies at the bottom of its capacious chamber, which is principally occupied by a loose cellular tissue, situated, like the arachnoid, between the pia mater and the dura mater, and having its cells filled with an oily fluid, or sometimes, as in the sturgeon, by a compact fat (xxiii. t. i., p. 309). Now, this condition of the envelope of the brain is not only like the fibrous tissue and squamous sutures of the ever-growing cranial bones, related to the requisite proportions of the fore part of the fish for facilitating its progressive motion, but it is one which no embryo of a higher animal ever presents; it is as peculiarly ichthyic, as it is expressly adapted to the exigencies of the fish.

It has been held that confluence of distinct bones is a consequence of high circulating and respiratory energies; yet, the anchyloses of the supra-occipital, parietal, and frontal, above the cranium, and of the basi-occipital, basi-sphenoid, and pre-sphenoid, below the cranium in *Lepidosiren*, and the constant confluence of the posterior and anterior basi-sphenoids in all bony fishes, disprove the constancy of the supposed relationship, and lead us to look for other explanations of such coalescence of primitively or essentially distinct bones. We shall find a final cause for the rapid consolidation and union of the elongated bodies of the two middle cranial vertebræ of fishes in the necessity for strength in the basis of that part of the skull, from the sides of which the large and heavy mandibular and hyoid arches, and their appendages are to be suspended, and to swing freely to and fro. The posterior and anterior sphenoids continue distinct bones in all mammalia, during a period of life at which they form one continuous bone in fishes.

The flattened form of the frontal and parietal bones in osseous fishes has been associated with the small development of the brain which they protect; but observe how they would have impeded the progress of the fish, had they been expanded into the dome-shaped vault which arches over the skull of Birds and Mammals. There was no need of that development in fishes; but we must not overlook the fact, that its very absence is a perfection in their structure,—an adaptation to their sphere and mode of locomotion.

The loose connections of most of the bones of the face may likewise remind the homologist of their condition in the imperfectly developed skull of the embryos of higher animals; but this condition is especially subservient to the peculiar and extensive movements of the jaws, and of the bones connected with the hyoid and branchial apparatus.

Not any of the limbs, properly so called, of fishes, are prehensile; the mouth may be propelled and guided by them to the food, but the act of prehension must be performed entirely by the jaws. Hence, in many fishes, both upper and lower maxillary bones enjoy movements of protraction and retraction, as well as of opening and shutting. The firm connections of the upper jaw, and wedged fixity of the bone suspending the under jaw, which characterize the higher Reptiles and Mammals, would be imperfections in the Fish; in which, therefore, such characters are not only absent, but special development in the opposite direction not unfrequently goes so far as to produce the most admirable mechanical adjustments of the maxillary apparatus, compensating for the absence of hands and arms like those which have been exemplified in the instance of the *Epibulus insidiator* (p. 108, fig. 37.)

We must guard ourselves, however, from inferring absolute superiority of structure from apparent complexity. The lower jaw of fishes might, at first view, seem more complex than that of man, because it consists of a greater number of pieces, each rumus being composed of two or three, and sometimes more separate bones. But, by parity of reasoning, the dental system of that jaw might be regarded as more complex, because it supports often three times, or ten times, perhaps fifty times, the number of teeth which are found in the human jaw. We here perceive, however, only an illustration of the law of vegetative repetition as the character of inferior organisms; and we may view, in the same light, the multiplication of pieces of which the supporting pedicle of the jaw is composed in fishes. But the great size, and the double glenoid or trochlear articulation of that pedicle, are developments beyond, and in advance of, the condition of the bones supporting the lower jaw in mammalia, and relate both to the increase of the capacity of the mouth in fishes for the lodgment of the great hyoid and branchial apparatus, and to the support of the opercula or doors which open and close the branchial chambers. The division of the long tympanic pedicle of osseous fishes into several partly overlapping pieces adds, to its strength, and by permitting a slight elastic bending of the whole, diminishes the liability to fracture. The enormous size, moreover, of the tympano-mandibular arch, and of its diverging appendages, contributes to ensure that proportion of the head to the trunk which is best adapted for the progressive motion of the fish through the water. But without the admission and appreciation of these preordained adaptations to special exigencies in the skeleton of fishes, the superior strength and complex development of the tympanic pedicle, and its appendages, would be inexplicable and unintelligible in this lowest and first-born class of Vertebrate animals.

In contrasting the skeletons of the Fish and Mammal, with reference to hypothetical secondary origins of organic species; such, for example, as that of transmutation and progressive ascent of specific forms, the vast disparity of the hyoidean arch, in point of size, complexity, and strength, both intrinsic, and as due to its connections, must not be overlooked. Its small size, simple structure, and loose suspension in the flesh, have led to its being reckoned in Anthropotomy as a single bone; and it is rarely preserved in the artificial skeletons of man or beast; whilst, if absolute and relative magnitude, complexity of structure, and importance of function, are tests of the grade of organization of a part, the progress of development must be held to have been reversed in respect of the hyoid arch; which, with its appendages, offers the highest grade in fish, and the lowest in man. And why this great difference—this striking exception to the general condition of the ichthyic organization? It is explicable only on teleological principles. It is true the Fish tastes not with its tongue, neither does it speak; the sole function of the human tongue bone, which is performed by that of the fish, is that which is in subserviency to deglutition. But this function is not in relation to food alone; all the mechanical part of breathing in the fish is a modified act of swallowing. The hyoid arch is the chief point of suspension of the visceral arches which support the gills; and the branchiostegal membranes, stretched out upon the diverging rays of the hyoid arch, regulate the course and exit of the respiratory currents; thus the mechanical functions of the thorax of the air-breathing classes are transferred to the hyoid arch and its appendages in fishes.

By the retraction of the hyoid arch, the opercular doors are forced open, and the branchial cavity is widened; whilst all entry from behind is prevented by the branchiostegal membranes, which close the posterior branchial slits; the water, therefore, enters by the gaping mouth, and rushes through the sieve-like interspaces of the branchial arches into the branchial cavity; the mouth then shuts, the opercular doors press upon the branchial and hyoid arches, which again advance forwards, and the branchiostegal membranes being withdrawn, the currents rush out at the open posterior branchial orifices. These functions are the true condition of the high development of those hyoides in fishes.

I have noticed the great development, the persistence, and ossification of the branchial arches in connection with the transitory manifestation of cartilaginous branchial arches in the larvæ of the Batrachia; this is one of those similarities that has led to the metaphorical expressions of "gigantic tadpoles," as applied to fishes. But we see how admirably the branchial arches are adapted to the aquatic respiration of the fish, by their advance to a grade of development, which they are never destined to attain in the frog. Observe their firm ossification, their elastic joints, the sieve-like valves developed from the side next to the mouth, prearranged with the utmost complexity and nicety of adjustment to prevent the entry of any particle of food or other irritating matters into the interspace of the tender, highly vascular, and sensitive gills. Observe, also, how the last pair of these arches is reduced to the capacity of the pharynx which it surrounds, how it is thickened in order to support teeth of multiform character, according to the nature of the food; in short, converted into an accessory pair of jaws, and the most important of the two. In no other Vertebrate animals is the mouth provided with maxillary instruments at both fore and hind apertures; in no other part of the ichthyic organization is the special divergence from any conceivable progressive scale of ascending structure culminating in Man so plainly marked as this is.

All writers on Animal Mechanics have shewn how admirably the whole form of the fish is adapted to the element in which it lives and moves; the viscera are packed in a small compass, in a cavity brought forwards close to the head; and whilst the consequent abrogation of the neck gives the advantage of a more fixed and resisting connection of the head to the trunk, a greater proportion of the trunk behind is left free for the development and allocation of the muscular masses which are to move the tail. In the caudal which, is usually the largest portion of the trunk, transverse processes cease to be developed, whilst dermal and intercalary spines shoot out from the middle line above and below, and give the vertically extended, compressed form, most efficient for the lateral strokes, by the rapid alternation by which the fish is propelled forwards in the diagonal between the direction of these forces. The advantage of the biconcave form of vertebræ with intervening elastic capsules of gelatinous fluid in effecting a combination of the resilient with the muscular power, is still more obvious in the Bony Fishes than in the shark.

You may be reminded that all the vertebræ of the trunk are distinct from one another at one stage of the quadruped's development, as in the fish throughout life; and you might suppose that the absence of that development and confluence of certain vertebræ near the tail, to form a sacrum, was a mark of inferiority in fishes. But note what a hinderance such a fettering of the movements of the caudal vertebræ would be to creatures which progress by alternate vigorous inflections of a muscular tail. A sacrum is a consolidation of a greater or less proportion of the vertebral axis of the body, for the transference of more or less of the weight of the body upon limbs organised for its support on dry land; such a modification would have been useless to the fish, and not only useless, but a hinderance and a defect.

The pectoral fins, those curtailed prototypes of the fore-limbs of other Vertebrata, with the last segment, or hand, alone projecting freely from the trunk, and swathed in a common undivided tegumentary sheath, present a condition analogous to that of the embryo buds of the homologous members in the higher Vertebrata. But what would have been the effect if both arm and forearm had also extended freely from the side of the fish, and dangled as a bony, flexible, many-jointed appendage in the water? This higher development, as it is termed, in relation to the prehensile limb of the denizen of dry land, would have been an imperfection in the structure of the creature which is to cleave the liquid element; in it, therefore, the fore-limb is reduced to the smallest proportions consistent with its required functions; the brachial and antibrachial segments are abrogated, or hidden in the trunk; the hand alone projects and can be applied, when the fish darts forwards, prone and flat by flexion of the wrist to the side of the trunk; or it may be extended at right angles, with its surfaces turned forwards and backwards, so as to check and arrest more or less suddenly, according to its degree of extension, the progress of the fish; its breadth may also be diminished or increased by approximating or devaricating the rays. In the act of flexion, the fin slightly rotates, and gives an oblique stroke to the water. For these functions, however, the hand requires as much extra development in breadth, as reduction in length and thickness; and mark how this is given to the so-called embryo or rudimental fore-limb; it is gained by the addition of ten, twenty, or it may be even a hundred digital rays, beyond the number to which the fingers are restricted, in the hand of the higher classes of Vertebrata. We find, moreover, as numerous and striking modifications of the pectoral fins, in adjustment to the peculiar habits of the species in Fishes, as we do of the fore-limbs in any of the higher classes. This fin may wield a formidable and special weapon of offence, as in many Siluroid fishes. But the modified hands have a more constant secondary office, that of touch, and are applied to ascertain the nature of surrounding objects, and particularly the character of the bottom of the water in which the fish may live. You may witness the tactile action of the pectoral fins when gold fish are transferred to a strange vessel; their eyes are so placed as to prevent their seeing what is below them; so they compress their air-bladder, and allow themselves to sink near the bottom, which they sweep, as it were, by rapid and delicate vibrations of the pectoral fins, apparently ascertaining that no sharp stone or stick projects upwards which might injure them in their rapid movements round their prison. If the pectorals are to perform a special office of exploration, certain digits are liberated from the web, and are specially endowed with nervous power for a finer sense of touch, as we see in the gurnards, where they compensate for the loss of the tactile property consequent on the hard covering of the exterior of the mouth in these mailed-cheeked fishes (*Joues Cuirassees*, Cuv.)

Certain Lophioids living on sand-banks that are left dry at low water, are enabled to hop after the retreating tide, by a special prolongation of the carpal joint of the pectoral fin, which fin, in these "frog fishes," projects like the limb of a terrestrial quadruped, and presents two distinct segments clear of the trunk.

The sharks, whose form of body and strength of tail enable them to swim near the surface of the ocean, are further adapted for this sphere of activity, and compensated for the absence of an air-bladder, by the larger proportional size and strength of their pectoral fins, which take a greater share in their active and varied evolutions than they can do in ordinary fishes.

The flat-bodied Rays, equally devoid of an air-bladder, and with a long and slender tail deprived of its ordinary propelling powers, grovel at the bottom; but have a still greater development of the hands, which surpass in breadth the whole trunk, and react with greater force upon it in raising it from the bottom, by virtue of a special modification of the scapular arch, which is directly attached to the dorsal vertebræ.

Nor is the pectoral member restricted in length where its office in subserviency to the special exigencies of the fish, demands a development in that direction; the fingers of the *Exocœtus or Dactylopterus*, are as long, and the web which they sustain as broad, as in the expanded wing of the flying mammal. Everywhere, whatever resemblance or analogy we may perceive in the ichthyic modifications of the vertebrate skeleton to the lower forms in the embryos of the higher classes, we shall find such analogies to be the result of special adaptations for the purpose or function for which that part of the fish is designed.

The ventral fins or homologues* of the hind legs are still more rudimental—still more embryonic, having in view the comparison with the stages of development in a land animal—than the pectoral fins; and their small proportional size reminds the homologist of the later appearance of the hind-limbs in the development of the land Vertebrate. But the hind-limbs more immediately relate to the support and progression of an animal on dry land than the fore-limbs: the legs are the sole terrestrial locomotive organs in birds, whose fore-limbs are exclusively modified, as wings, for motion in another element. The legs are the sole organ of support and progression in Man, whose pectoral members or arms are liberated from that office, and made entirely subservient to the varied purposes to which an inventive faculty and an intelligent will would apply them. To what purpose, then, encumber a creature always floating in a medium of nearly the same specific gravity as itself, with hind-limbs? They could be of no use: nay, to creatures that can only attain their prey, or escape their enemy, by vigorous alternate strokes of the hind part of the trunk, the attachment there of long flexible limbs would be a grievous hinderance, a very monstrosity. So, therefore, we find the all-wise Creator has restricted the development and connections of the hind-limbs of Fishes to the dimensions, and to the form, which, whilst suited to the limited functions they are capable of in this class, would prevent their interfering with the action of more important parts of the locomotive machinery.

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* *Homologue*, intimates the same organ in different animals under every variety of form and function. *Analogue*, a part or organ in an animal which has the same function as another part or organ in a different animal.

In most fishes the ventral fins merely combine with the pectoral fins in raising, and in preventing, as outriggers, the rolling of the body; but some very interesting modifications of the ventral fins in relation to particular habits of certain species, may be noticed. In the Blennies, the Forked Hake (*Phycis*), the Forked beard (*Raniceps*), and some other fishes, the ventral fins are reduced to filamentary feelers. In the Lump-suckers (*Cyclopterus*), the ventrals unite together, and combine with part of the pectorals to form a sucking disc or organ of adhesion below the head, just as the opercular and branchiostegal fins are united together to form the gill-cover. In the long-bodied and small-headed abdominal fishes, the ventrals are situated near the anus, where they best subserve the office of accessory balancers; in the large-headed thoracic and jugular fishes, the loose suspension of these fins, and the absence of any connection with a sacral part of the vertebral column, permits their transference forwards to aid the pectoral fins in raising the head.

The following short account of some experiments upon fish, made for the purpose of ascertaining the use of their fins, I give in the words of their gifted describer Paley, to whom comparative physiology owes many beautiful accessions to its teleological applications.

"In most fish, beside the great fin—the tail, we find two pairs of fins upon the sides, two single fins upon the back, and one upon the belly, or rather upon the belly and the tail. The balancing use of these organs is proved in this manner: Of the large-headed fish, if you cut off the pectoral fins, that is, the pair which lie close behind the pectoral gills, the head falls prone to the bottom; if the right pectoral fin only be cut off, the fish leans to that side; if the ventral fin on the same side be cut away, then it loses its equilibrium entirely; if the dorsal and anal fins be cut off, the fish reels to the right and left; when the fish dies, that is, when the fins cease to

play, the belly turns upwards. The use of the same parts for motion is seen in the following observation upon them when put into action. The pectoral, and, more particularly, the ventral fins, serve to raise and depress the fish: when the fish desires to have a retrograde motion, a stroke forward with the pectoral fin effectually produces it: if the fish desires to turn either way, a single blow with the tail sends it round at once: if the tail strike both ways, the motion produced by the double lash is progressive, and enables the fish to dart forward with an astonishing velocity. The result is not only in some cases the most rapid, but in all cases the most gentle, pliant, easy, animal motion, with which we are acquainted. However, when the tail is cut off, the fish loses all motion, and it gives itself up to where the water impels it. The rest of the fins, therefore, so far as respects motion, seems to be merely subsidiary to this. In their mechanical use, the anal fin may be reckoned the keel; the ventral fins, outriggers; the pectoral fins, the oars; and if there be any similitude between these parts of a boat and a fish, observe that it is not the resemblance of imitation, but the likeness which arises from applying similar mechanical means to the same purpose."—(xli. p. 257.)*
Owen's Lectures on Comparative Anatomy, vol. ii., p. 145.

On the Small Temporary Glaciers of the Vosges. By EDWARD COLLOMB.

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The winter of 1844–45, which has been remarkable for the large masses of snow which the west wind heaped up on the slopes of some of our mountains (the Vosges),—masses sometimes from 15 to 20 metres in thickness,—gave rise to small temporary glaciers, possessing some of the properties of large glaciers, and, among others, a locomotive movement. The duration of these glaciers was limited to a few months; by the end of July and beginning of August, they had completely disappeared. During this interval, we have had an opportunity of making many observations in reference to the transformation these masses underwent between the winter and summer.

The first fact that struck us, is the tendency of the snow, when in a large mass, to form itself into stratified layers during the time of melting. The second fact, is the *proper move-*

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* See also Carlisle Phil. Trans. 1806, p. 3.

ment observable in all the masses of snow which still exist on our mountains in the spring and during a part of the summer.

This movement, which does not proceed from sliding on an inclined plane, but from an *expansive motion* (*mouvement d'expansion*) arising from its partial conversion into ice, assimilates these masses to true glaciers, although, it is true, in very small proportions.

On the 6th April 1845, we explored the bottom of the valley of Wildenstein (Haut-Rhin). A few weeks before, a pretty large avalanche had detached itself from the Col du Rothenbach, and, after a transit of about 1000 or 1200 metres, had accumulated a great quantity of trees, blocks, clay, sand, and considerable masses of snow at the foot of the *couloir*.

A note relating to this fact, inserted in the Comptes Rendus of the Institute, states the following particulars :—

" At the points where the avalanche has collected the snow in a great mass, and when the rivulet has formed, by its melting, pretty distinct sections, we have studied, with ease, the different stratifications of the snow, the passage of the snow into névé, and from névé into ice, such as MM. Agassiz and Desor observed in the high regions of the Alps. In a thickness of some metres, the strata succeed each other in the following order :—

" Minute névé, or powdery snow.
Large-granular névé.
Ice of névé.
Vescicular ice,
Compact ice, resting on the ground.

" Before the snow could become stratified in this manner, it must have been subjected to many atmospheric influences since its accumulation ; among others, a moderate heat during the day, and cold night, circumstances which, by causing a partial melting, have allowed the mass to imbibe water, and then to congeal.

" On the declivities where we observed them, the masses of névé having imbibed water, already exhibited a motion of their own. Of this we satisfied ourselves, by an attentive examination of the different obstacles which opposed the descending progress of the névé.

"The melting caused by the heat radiated from the trunk of a tree, ought to have formed a circle concentric to the tree, as is seen on a plain; but on an inclined plane, the circle produced is not concentric, but eccentric; the movement impressed on the névé from above, makes it descend till it touch the trunk; the moss and lichens with which it is sometimes covered, are bruised and rubbed on this side, while, on the opposite surface of the trunk, these cryptogams have preserved all the delicacy of their forms. We have remarked this arrangement of the eccentric melting of the snow around many hundreds of trees. Whether the ground be exposed to the north or south, the same thing takes place."*

On the following months we continued our examinations at different points of the Vosges chain, where the snow was still accumulated in sufficiently large quantities to enable us to carry out the same observations. A second note, inserted in the *Comptes Rendus* of the Institute, gives the following details:—

"On the 15th of June, there were still numerous patches of snow on the eastern sides of the Vosges, from the Rothenbach (1319 m.), as far as Hoheneck (1366 m.), in the bottom of the small valley of Munster. The presence of snows in this locality, at this season of the year, is explained by the manner in which they have been driven thither in winter by the violent west winds. These winds forced them to accumulate in the circuses of the opposite sides, which separate the culminating points.

"Some of these patches were at this time reduced to a few square metres; the largest still presented a surface from 4000 to 5000 square metres. The declivities on which they lay had a great inclination, a mean of upwards of 45 degrees; they were true precipices.

"Upon examining these snows attentively, I found the upper part formed of large grains of névé. It was from 25 to 30 centimetres thick, offering considerable resistance, and pretty hard; the foot sunk very little in it. Immediately below, a layer of névé ice commenced, which became harder and harder the farther one penetrated forwards into the mass. By striking with the point of a hammer, morsels not larger than the hand were displaced; then by digging downwards, it was ascertained, from the resistance offered, that this ice became quite hard.

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* Ed. Collomb, Lettre a M. E. de Beaumont (*Comptes Rendus*, t. xx., p. 1305).

"This snow was therefore stratified, as we have already seen, in the month of April; only the powdery snow of the surface had disappeared, and then only remained successive beds of

"Large granular névé;
The ice of névé;
Vescicular ice;
Compact ice.

The thickest part of these masses was still, in the month of June, from 4 to 5 metres.

"The passage of large granular névé into névé ice is easily observed; the transition is sudden. The névé is formed of grains or granular concretions sometimes many millimetres in diameter, translucent, brilliant, and moistened with water; they readily become agglutinated. The pressure, in the hand, of the névé for a minute, is sufficient to convert it into ice,—this névé ice being nothing else, as M. Charpentier has already remarked, than the union or soldering together of a certain number of these grains or concretions.

"But the transition of the vescicular ice into compact ice is not so easily discerned. These two species of ice pass insensibly into one another.

"On the 15th of June, the temperature of the surrounding air, at 2 o'clock P.M., on the eastern declivities of these mountains, which do not rise above 1200 or 1300 metres above the level of the sea, rose from 13 to 20 centigrade degrees in a clear day. This temperature ought to have caused a rapid melting, followed by a flow of water from the lower part of these sloping patches of névé. On the day in question, however no water was discharged; the névé, no doubt, disappeared, during fine weather, only by evaporation. The heat of the ground does not assist in melting, for the layer of ice at the bottom is soldered to the grass and mosses on which it rests. On the parts recently left by the ice, these vegetables appear pressed down and flattened in the direction of the declivity, as if a heavy roller had passed over them.

" What particularly distinguishes these old snows from such as exist only in the winter, is, that they share in the properties of the Swiss glaciers; they possess a locomotive movement which transforms them into true glaciers, although of small size. This movement of the névé, which we had formerly remarked in the month of April, we again witnessed at Hoheneck in a very striking manner. When the small glacier rests upon a pointed rock, and this rock forms a promontory, the distance between the rock and the névé sometimes extends to upwards of a metre in a longitudinal direction; while, in the lateral sense, the névé touches the rock, or is distinct from it on a few centimetres. This disposition appears wherever any obstacle fixed in the ground arrests the descending progress of the névé.

" The large glaciers have the power of transporting the matters which lie on their surface, and forming moraines; our microscopic glaciers of the Vosges, having the power of motion, likewise carry along with them the objects which happen to be on their surface. The only difference is, that these objects, instead of being blocks of 1000 cube metres, are composed of small fragments of a few square millimetres in size. The locomotive principle is the same, the only difference being in the size of the masses.

" Accordingly, on the surface of our old snows, we observed, on the 15th of June, coloured bands of a deep grey on a white ground; on examining these close at hand, they were found to be composed of exceedingly minute debris, earth and sand, accompanied by the vegetable detritus which follow the movement of the mass of névé, and form a ribbon of a muddy hue, turning round the rocks, and following their sinuosities to the distance of many metres.

" This ribbon indicates a moraine in infinitesimal proportions; its breadth varied according to the position of the masses of névé, the medium being from 6 to 8 centimetres. Each mass of snow of any dimensions, that is to say, of at least 1000 square metres, was provided with one. That which I examined most particularly at Hoheneck, was formed of debris of earth and rock, detached from an abrupt projection; they had rolled on the névé, and been carried to the distance of some metres, assuming an elongated form, analogous to that of large moraines.

"It is easy to climb up these snows, even when the planes are inclined upwards of 45 degrees; their surface is unequal, as if formed of small waves, the hollow part of which serves as a support, or the step of a ladder to fix the feet.

"If we examine a longitudinal section in one of these small glaciers, we observe that the upper part terminates in a narrow point, and of a cavernous form; the cavity which separates the névé from the ground, is everywhere upwards of a metre in height by 5 or 6 in depth, in the direction of the declivity; while the lower part, on the contrary, is heaped upon itself in the form of a sack. This arrangement is general; it arises, no doubt, from the motion of the névê, which naturally causes it to accumulate in this direction. What leads me to this belief is, that, in the months of February and March, a few weeks after the fall of these snows, when they were newly heaped up by the wind, in circles similar to those of Hoheneck, before the movement began to take place, the melting and evaporation not yet being sufficient to produce that effect, the section of the masses was of an opposite form. The greatest thickness of the snow was always found, at this season of the year, in the upper part.

"This total change in the whole form of these masses, produced in the interval between winter and summer, indicates more than a natural tassement; it evidently implies a movement in the interior of the mass."*

About the same period, the snows accumulated on the declivities of Drumont (1208 m.), towards the contre-fort, which separates the valley of Schliffels from that of Urbès, had acquired such a motion, that they broke down a great number of trees, especially young beeches; other trees, of a more pliant nature, were curved and bent to the earth by the strong pressure of the masses of névé from above. The strongest trees had resisted; but the marks left on the bark enabled us to trace the line to which the friction of the snow extended.

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* Ed. Collomb on certain Movements observed in the Snows of the Vosges before being completely melted (*Comptes Rendus of the Institute*, t. xxi., p. 327).

This movement,—this pressure—caused by the snow, does not exist in winter.

The trunks of trees are not compressed and broken; but, when the season, by becoming warmer, causes a partial melting of the snow, penetrates it with water, and changes it into névé and ice. In such cases, the movement exists everywhere, as well on gentle slopes, where the inclination does not exceed 6° or 7°, as on inclined planes of 45°. If this movement were caused by sliding, it would be much more rapid on a steep declivity than upon an almost horizontal plane.

We continued these observations a little later. On June 22d, 1845, on the ballon of Servances, there were still five or six patches of snow on the northern slope of this mountain (1120 m.). The largest of these patches covered a surface of about a hectare, with a very steep declivity. The flow of water from this mass was very small; although much rain had fallen the preceding night, the water that escaped was evidently not in proportion with that which such a quantity of snow ought to produce in the state of temperature which then prevailed. These small glaciers act like a large sponge, absorbing the water, retaining it, and solidifying a portion of it.

The movement of the mass was evidently indicated, as at Hoheneck, by its longitudinal distance from the rock, and its lateral approach to it.

This year (1846), it was my intention to continue experiments on these small glaciers with the greatest care, to measure their movements accurately, and to calculate the loss of their surface in a given time; but having failed in accomplishing this, I must confine myself to a few observations on their stratification. In the months of January and February, scarcely any snow fell. By the 5th of March, the ordinary season of deep snow, we traversed the summits of the Vosges along with MM. Agassiz, Desor, and Dollfus, without meeting with large masses, except on the southern slopes of the Rothenbach; and at this season of the year the movement was not perceptible. At the same time, the beds of snow were stratified, and had passed into the state of névé; but the part of névé resting immediately on the ground, had not been converted into ice. The alternations of heat and cold were not sufficiently great in the month of March to produce the circumstances favourable for converting the inferior névé into ice. There were thin layers of ice interposed, but they did not touch the ground. The sections we made in the mass presented the following successive layers:—

Néve.
Ice.
Névé.
Ice.
Névé.
Ground.

The layer of ice lying between the névé was very compact and compressed almost like plate-glass, from 5 to 10 mm. in thickness. This layer no doubt corresponds to the time of a fall of snow followed by verglas. I account for this phenomenon in the following way: An excessively thin superficial layer of clear ice being formed, a new fall of snow took place, then this snow partly melted in order to be transformed into névé; but during this operation, the verglas did not act as a filter, but, on the contrary, arrested the waters of infiltration; the latter became frozen, and formed a layer of ice from 5 to 10 mm. in thickness. May we not find in this the origin of the blue and white veins of the large glaciers, respecting which so much discussion has taken place?

Two months later, on the 10th May, the Drumont still retained, near its summit, a series of spots of snow, some metres in thickness, leaning against a slope of 30 or 35 degrees. These snows were in motion. The fact was evident; they had pressed strongly against the trunks of the trees that stood in their way. The stratification of these snows was in a very advanced state, the part resting on the ground being completely transformed into ice. By cutting sections in this mass, the followings beds were noticed:—

Névé,
Névé ice,
Vescicular ice.

At other places, the veins of mirror ice which we had remarked on the Rothenbach, two months before, still existed. The layers were then found to succeed each other in the following order :—

Névé,
Vein of mirror ice,
Névé ice,
Vescicular ice.

The vescicular ice, resting on a turfy soil, was covered on its lower part with long grooves and striæ, which corresponded to dry stalks of grass which it had compressed and carried along with it, thus receiving the impression of them. Many of these stalks still adhered to the ice. At some places this vescicular ice had passed into the compact state.

These masses of névé were all—at different degrees of depth—at a temperature of 0° cent.

The surrounding air, in the shade at mid-day, under a cloudless sky, was that day about $+15{\cdot}5$ degrees.

The temperature of the ground, at the depth of 10 centimetres, even under the masses of névé, was some degrees above zero. However, the ground itself, the turf was not wet with the ice, but only slightly humid, almost dry. By the 24th of May all these masses had disappeared.

In all the masses of névé in motion which I examined among the Vosges in 1845 and 1846, I never observed that the slope of the ground was of much importance in the phenomenon ; but, as I made no direct experiments on this subject, it is only by way of approximation that I judge of the fact. I may remark, however, that in the forests situate between the Valley of Urbès and that of Schliffels, where the declivities are small, the movement of the small glaciers has committed great ravages last year ; it has broken and bruised a great number of trees ; the movement was as decided there as on the slopes of 45 degrees on the eastern side of Hoheneck. I often visited our summits in the midst of winter, and I never saw any motion in the snow at that season, whatever might be the degree of inclination in the slopes.

Phenomena peculiar to great glaciers, such as crevasses, tables, and needle-like prominences, &c., are not here observed; but it is not a little remarkable, that our small model glaciers possess one of the most characteristic properties of these masses of ice, namely motion.*

Memoir on the Bones found in the Stratified Gravels in the vicinity of Mattegnin, Canton of Geneva. By Professor F. J. PICTET.

Some years ago, we were informed by M. Fazy, counsellor of State, then Mayor of the community of Meyrin, that numerous bones had been found in digging a gravel quarry near Mattegnin. The importance of fossil organic remains, in order to determine the relative age of formations, is so great, that we immediately took means to have all these bones carefully collected. M. Fazy himself superintended the workmen, and, thanks to his enlightened care, our museum is now possessed of a considerable collection.

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* The fact of the transformation of masses of snow into ice and into small temporary glaciers, has likewise been observed among the Alps. I find, in the *Bulletin de la Société Géologique*, which gives an account of the extraordinary meeting at Avallon, a note of the Canon Gall, containing very interesting observations on the erratic phenomenon of the Valley of Aoste. This note contains the following paragraph on the subject which now occupies our attention.

"With regard to the rocks found on the passage of the avalanches, they present a surface rather rubbed than polished by the friction or collision of wood and pebbles, or stones carried along by the huge masses of snow; I never saw any striæ. I have said *the passage;* for the rocks found in the bottom when the avalanche is arrested, may be polished and striated, since the avalanche, when it is very considerable, and lasts a long time without melting, assumes the character *of a small temporary glacier*. We observe in it almost the same revolutions; the snow becomes ice, it presents various vaults at its base, its surface is crevassed, &c. This I have remarked this year at Pre-Saint-Didier, at the avalanche of Champex, which annually descends from a valley on the north-west of Mount Grammont, and which, at the end of the month of July, was still, judging by the eye, about 6 metres above the ground."—*Bulletin de la Société Géologique*, 2d Series, vol. ii., p. 730.

The first point to establish was their geological position.*

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* Professor A. Favre has communicated to me some observations made by him on the repository of these fossils, and the nature of the gravels which contain them.

Note on the gravels containing the fossil bones.

At first sight, the gravels in which these bones occur, might appear to belong to the lower stage of the diluvian formation of Geneva; but a more careful examination of the neighbourhood of the hamlet of Mattegnin, shews that the gravels in question form part of the upper stage of this formation. The following is the appearance presented by a section of the gravel quarry of Mattegnin, going from the upper part downwards.

1*st*, One or two feet of vegetable earth.

2*d*, Sandy clay, forming a paste with water, named *terre a pisè*, *terre blanche* or *glappe*, by the peasants, who use it for building. It contains a pretty considerable quantity of rolled pebbles, some of which belong to the crystallized formations of the Alps, such as granites, protogines, quartz, crystalline slates, &c., and others consist of limestones, in general of a deep colour. The latter are frequently striated or rayed on the surface, which makes them resemble the rocks polished by glaciers. This bed is variable in thickness; in the quarry it does not exceed 18 inches, while, at a few hundred metres from that place, near Meyrin, the peasants affirm that it is 10 feet.

3*d*, Gravel or *beton*, more or less tenaceous, according to the greater or less quantity of stalactitic cement, which solders the pebbles to each other. These pebbles vary greatly, but they are not striated on the surface. We notice everywhere beds of sand, very nearly horizontal, which divide this mass of gravel in a more or less regular manner. In the upper parts, these gravels are of ferruginous tints; it is in this bed that the bones are found. Thickness from 10 to 12 feet.

4*th*, Coarse clay, remarkably tenacious, forming a paste with water, mingled with a great quantity of different kinds of pebbles. The calcareous pebbles are striated. Although reddish, this bed is often called *white earth* by the peasants. Thickness not ascertained.

The gravel containing the bones, therefore, rests upon an argillaceous earth containing striated pebbles. This, as I have mentioned elsewhere (*Considerations geologiques sur le mont Salève et sur les terrains des environs de Genève*, 1843, p. 74), is one of the principal characters of the cataclystic diluvian formation. This gravel, then, forms part of this formation, as I undertake to prove. In traversing the environs of Mattegnin, we may convince ourselves that it is a local deposit much less extended than the ancient alluvion which it resembles, and, in its geological position, very like the deposits of sand at Frontenex, Malagnou, Çartigny, &c., cited by M. Necker, (*Etudes Geologiques dans les Alpes*, i. 248, 259, &c.)

With regard to the ancient alluvion which extends horizontally along the

It was of importance to know whether they had been deposited at the same time as the gravels, or whether they were buried by some later and more special cause. Discoveries similar to those of these remains, made in different parts of our Canton, had, in fact, often remained unnoticed, because they were conceived to be the result of events altogether modern.

M. Necker, in particular, in his *Etudes Geologiques sur les Alpes* (t. i., p. 262), speaks of the bones of oxen found in sand near Chaucy. This skilful geologist is disposed to think that they owe their origin to this, that "in times, no doubt within the limits of history, but very remote, to judge by the condition of the bones, and on the occasion of some great disease among cattle, entire herds of large cattle had been buried in these sands."

But this explanation cannot apply to the bones of Mattegnin. I have, at different times, visited the quarry where they are found, and have myself taken out many skeletons. I have constantly found them at a depth of, at least, seven or ten feet below the part of the soil influenced by cultivation, and under beds of gravel very distinctly and regularly stratified. I have never seen the direction and continuity of the beds in the least degree altered above these organic remains.

It must, moreover, be remarked, that the whole plain of Mattegnin is covered with diluvian gravels, and that there are neither great undulations, sections, nor water-courses, which might derange the superposition of the beds. The gravels which enclose the bones are composed of small stones, more or less flattened, disposed in very obvious strata, alternating with very fine sand. It is likewise evident, that these gravels, situate 60 metres above the lake, and 70 metres above that part of the Rhone which is nearest Meyrin, re-

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bottom of the valley of Geneva, without ever rising either against the Jura or the Alps, perhaps we might regard it as belonging to the upper tertiary formation, and as being contemporaneous with the ancient *terrain* of Bresse, described by Elie de Beaumont. I make this supposition, but with much doubt, for the characters which are drawn from the composition of this formation, and the presence of a small quantity of lignite, are not, I confess, sufficiently decided altogether to authorise it.

mote from all streams of water of any consequence, cannot have been deposited by a special accident, and that they owe their origin to the general cause which deposited the alluvial formation of the basin of the lake of Geneva.

I think, therefore, that we may consider it as a fact, that the animals whose skeletons are found there, have lived in our valley during the period immediately preceding the deposition of this diluvian formation.

I shall afterwards shew that all these animals belong to the same species as those which now live in these same countries. From this circumstance, the discovery of these bones may appear of little interest, and it may perhaps seem, at first glance, that little instruction can be derived from the study of them. I shall endeavour, on the contrary, to shew that this fact, apparently unimportant, is, in reality, connected with one of the most essential laws of palæontology, and may influence the solution of the most delicate questions. But I must previously prove this identity of species; and I have devoted, to this comparison, the time and attention which this essential point seemed to merit, and which will form a foundation for the reflections which shall terminate this memoir.

[The original memoir here enters into details respecting these various bones, and the list of the species discovered. These gravels contain a *Sorex araneus*, the mole, martin, polecat, weasel, the fox, a rat (probably referable to the *Mus leucogastor*, Pict., and which seems to prove that this species had preceded man in our countries, and had not been imported by means of maritime commerce, like the common black and brown rats), the field mouse, domestic mouse, *Schermaus*, *Arvicola subterraneus*, the common campagnol, a pig, an ox, the chamois, a sheep or a goat, a passerine bird, the common toad, the green frog, and green lizard.]

The comparison of the fossil bones of our gravels with the skeletons of living species, as well as analogous facts, which may be collected even to the present time, prove, in my opinion, with complete evidence, that the animals which peopled our valley before the diluvian formations were deposited, are *identical with the* presently existing species. This conclusion is confirmed by the examination of fifteen species of mammifera, and three of reptiles. We may, in my opinion, draw from this the following consequences.

I can here find, in the first place, a proof of the law of the *permanence of species.* Among the principal arguments on which this important principle rests, that most frequently insisted upon is the comparison of the animals preserved in the ancient sculptures of Egypt with the existing species. When we perceive that the lapse of four thousand years has produced no modification of form, not even the slightest, on the skeletons of crocodiles, ibises, cats, ichneumons, &c., we are authorised to deduce from the fact the general law that the species invariably preserve their characters. The bones preserved in our gravels are undoubtedly still more ancient than those of Egypt, and, therefore, furnish a more positive proof of this. But what more particularly gives them a greater degree of interest, is what has been often alleged, namely, that, while we admit the permanence of species in the present state of the globe, we may likewise allow that, at periods of great geological changes, more active forces, and more intense atmospheric alterations, may have caused more important modifications in organism. The fossil species of our diluvian deposits have assisted at one of these geological changes; they have inhabited our valley before the deposition of the gravels, and yet they have continued unchanged until our own day. Do we not thus obtain, at least, a presumption, that it is the same in such cases in general; and may we not draw from this an argument in favour of the much more probable opinion, that great geological events have killed and destroyed the species, but could not produce any modification in their forms?

I believe, in the second place, that we may find in these facts, a proof that our diluvian deposits are more recent than those of the greater part of Europe. The bones which I have examined, belong, as I have stated, to the diluvian formations of our valley, and, yet all the species to which they can be referred are still alive. Now, the greater part of the European diluvian deposits enclose the bones of elephants, rhinoceroses, and other species now extinct. Such is likewise the case with the caves, in which occur bears and large feline animals belonging to lost species. From this, it appears to me probable, that the gravels of the greater part of Europe have been deposited, and the caverns been filled with the remarkable bones which characterise them, at a period anterior to that when our later arenaceous deposits were formed; and that, consequently, the soil of our valley is the produce of a geological event of a comparatively recent date.

I finally draw, from the study of these bones, a third conclusion, which is the confirmation of an opinion I have stated elsewhere (*Traitè èlèmentaire de Palèontologie.*) I am of opinion, that the era designated by geologists the diluvian epoch, ought not to be distinguished from the modern epoch, for I think it can be demonstrated that there are no changes of fauna between them, which changes are so remarkable in regard to the anterior epochs. It appears to me that the greater part of the species of the diluvian epoch still live in our own day. I do not admit that any have been created at the commencement only of the modern epoch. I think that the diluvian deposits of Europe have been formed by a series of partial occurrences, which have not interrupted, in a general way, the life and organization on the surface of the earth, but which have been limited to the destruction of a few species.

The corroboration of this manner of viewing the case, depends on a lengthened chain of facts, of which those I mention here form only a link, and for which I refer to geological treatises. My principal arguments are, in fact, drawn from the study of the fossil animals of the epoch named diluvian. The caves and gravel deposits of Europe, contain many skeletons of mammifera which cannot be distinguished from the actual species, and a very small number of lost species. Almost all the animals now inhabiting our countries have already existed at this epoch. The wolf, the fox, the mole, badger, &c., &c., of caverns, are identical with those now alive. The fossils of the Genevese gravels appear to me to furnish a new proof of this, for the identity of the species is, in this case, still more marked, and shews a period of transition between the age of the animals of caverns and modern times. No one, I think, entertains the idea of terminating the diluvian epoch immediately after the deposition of the mud of caverns, and to place that of the gravels of Switzerland in the modern epoch. The only means, therefore, of making these facts agree, is to unite, as I have said, the diluvian epoch to the modern.

It must, therefore, be admitted, that immediately after the last tertiary deposits, the modern epoch commenced; and that, at the beginning of this epoch, all the animals which now inhabit Europe were created, and also certain other species, such as the large carnivora of the caves, some kinds of antelopes, &c. Inundations, more or less general, probably produced in part by the last elevation of the Alps, have covered many parts of Europe at different times. The oldest are those which have filled the caverns; after them, come those that have deposited the gravels of France, and the most modern are such as have formed the gravels of our countries. These inundations, and perhaps other causes, have caused some species to disappear, as we notice some becoming extinct in the present day (such as the aurochs, &c.) The greatest number have survived, and form the presently existing European fauna.

This mode of viewing the question appears to me quite simple and natural. At the same time, I can perceive that objections may be made to it; and of these I foresee two, which I may answer beforehand.

The first relates to Man. The union of the diluvian and modern epochs seems to agree ill with his late appearance, and with the fact that his bones are not found in the cavern epoch. I have already noticed this objection in my Treatise on Palæontology, and called attention to the fact that I here speak of the diluvian epoch only in relation to Europe, and that it is necessary to distinguish the creation of man from his late appearance in this country. I believe that the human species has not been a witness of any of the great European inundations, and that it has not appeared there till after the deposit of the gravels. But there is nothing to indicate that man did not, at that epoch, inhabit the Asiatic Continent, which all agree in regarding as his cradle. I think that European geology is not called upon to occupy herself with the creation of man, and that, in our ignorance of the manner of what took place in Asia, there is no argument to be adduced against the idea which I have now brought forward.

It may be objected, in the second place, that the European fauna would have been too abundant, if we add to the existing species those which the different diluvian inundations have destroyed. I have already said, that these latter were by no means numerous; and if we compare the existing fauna of Europe with that of Asia and America, we will find it infinitely poorer in mammifera. What may have been created at the commencement of the diluvian epoch would still leave it inferior to them in this point of view.

These objections do not appear to me to shake the opinion which I here advance, and I think that we may consider it as very probable that the different diluvian formations enclose a series of faunas differing little from each other, a circumstance which proves that the diluvian and modern epochs have been in no way separated by an event comparable to those revolutions of the globe which, at different times, have extinguished existing species, in order to replace them by others wholly different.—(*Bibliothèque Universelle de Genève, N.* 7, *August* 1846.)

On Ground-Ice. By Mr Richard Adie, Liverpool. Communicated by the Author.*

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In the present communication, it is my wish to attempt to account for ground-ice, without the intervention of any hypothesis which attributes the ice found in the beds of streams to a process of formation *in situ*.

An attempt of a similar kind has already been made. Mr Knight, in his description of ground ice, says that spiculæ were floating in millions in the stream, and that on the faces of stones opposed to the current, where it was only of moderate strength, these spiculæ *accumulated*. The Rev. Mr Esdaile, in a most valuable paper printed in the 17th volume of this Journal, p. 167, quotes Mr Knight's description as almost amounting to demonstration of a theory he proposes, viz., that particles of hoar-frost, or frozen dew, falling into an ice-cold stream, get agglutinated to rocks or obstacles in the bed of it; and thus form a nucleus for that strange accumulation called ground-ice, which is found nowhere but in running streams.

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* Read before the Wernerian Natural History Society.

Mr Esdaile's explanation stops short after forming a nucleus for the ground-ice, withont informing us whether there is any actual growth of ice under the surface of the water. Now, if ice is supposed to grow through the freezing of water about tho obtained nucleus, then it would be the laws which produce this increase that would demonstrate the theory of the formation of ground-ice. But as I believe that evidence can be found to shew that in this climate there is no formation of ice under running water, and that if ice was so formed, it would be clear and hard; while the vast accumulations of soft ground-ice which are sometimes met with, are no other than particles of ice mixed up with ice-cold water, and submerged, in the same way that earth and sand are suspended during floods in the whole of the water of a river. The only difference is, that the superior power of adhesion among minute particles of wet ice, enables them to assume a position of repose for the time being, which is unnatural, or an inversion of hydrostatic laws, while the repose of the earthy particles is in obedience to those laws; and further, that a large portion of the materials of the so-called ground-ice is formed at the edges of streams, from whence, in favourable localities, it drops into the water and is carried down.

My friend Mr James Elliot junior, Hawick, has from time to time been in the habit of sending me descriptions of ground-ice in the river Teviot. In 1841, on 17th November, he writes he saw it in abundance, and that it was rare to find it so early. His descriptions confirm in a singular manner Mr Knight's observations as to millions of floating spiculæ; also as to their accumulating most on the sides of stones opposed to the current. Mr Elliot has often noticed that where the bed is covered with slimy matter, chiefly composed of the confervoideæ, there the ground-ice is in far the largest quantities; and in a few cases he found it beneath a surface ice covering. In the course of this correspondence we had often discussions as to the relative merits of the different theories offered for the explanation of the phenomenon; where Mr Elliot certainly took the lead in rejecting the received views,* when his own observations and remarks had gone far to give probability to an entangling hypothesis meeting all the facts of the case. I had often occasion to urge the examination of this theory, from a firm conviction that it would end in being established, although I was at the time unacquainted with Mr Knight or Mr Esdaile's writings. Since then I have had opportunities of making some observations, an account of which I now beg to offer.

In January 1843, I examined some ice formations on the Pentland Hills, near Edinburgh, which come strictly under the term ground-ice. Although they relate to ice in very different circumstances from what we find in streams, yet they bear on the question of nuclei for the crystals. After a showery day, the sky before midnight cleared, and a sharp frost suddenly set in; next day there were pools of water on the roads covered with cakes of ice ¼ of an inch thick. On the sides of the hills where the rain-water had been slowly oozing down, in the sheep-paths and places a little bare of vegetation, I observed innumerable ice-pillars rising, closely compacted in beds, to heights varying from ½ to 2½ inches; the sides of these pillars were white and striated, very much resembling the sections of the broken cakes of sal ammoniac of commerce, only not so solid in texture; they all carried capitals, composed chiefly of fragments of stone, but pieces of earth or heath, or in fact anything that could be raised, was elevated on these pillars. One small bed of them was crossed near the middle of the pillars with two parallel bands of coarse grains of sand; which appeared to me to shew that their growth took place from the ground, and that the bands might be accounted for by some brief meteorological change, checking twice the formation of the ice. The capitals appeared to be useful for the first start of the pillars, for there were none without them; but it is a gradual regulated supply of water which has the most essential part to perform; for where there is too much water the stones are frozen fast down and do not rise. These ice-formations are, I find, familiar to those who have walked on hilly undrained ground, during frosts. On crossing over to the south side of the hill, the sun suddenly emerged from behind a cloud, when it was interesting to observe the rapid decapitation of the ice-pillars which immediately commenced. The portions of soil and plants which here served to begin a process so active in ice-making, are never awanting in pools or running streams, where they may be expected to be efficient in forming scales or spiculæ. The overhanging banks of many mountain torrents may also often offer favourable places for soft ice so formed dropping into the stream. When the thickness of solid ice-cakes is compared with the length of the pillars, the rapidity of the latter in forming ice is apparent.

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* It is to M. Arago and Dr Farquharson's modes of accounting for ground-ice that allusion is here made.

In the vicinity of the village of Altcar, 12 miles north of Liverpool, there is a stream locally known by the name of the Altcar Brook, a tributary of the river Alt, well adapted for examining ground-ice in running waters of low velocity. The brook drains a flat open district adjoining the Irish sea; the ground is intersected by numerous open ditches holding large quantities of water, coloured by peat of a dark brown hue. The velocity of the current in the brook for two short distances near its termination is from 2 to 3 miles per hour, flowing smoothly along in a mud or sand bottom with small eddies. Plants occupy the bed in patches: these in the winter season are all covered with water to a depth of from 6 to 12 inches. The upper portion of this stream is a wide slow flowing ditch, as much exposed to the elements as the surface of the adjacent sea. In the second week of December 1846, I found abundance of ground-ice, on the third day of a moderate frost, assisted by a very dry wind. The temperature in the stream was 32° exactly; the air in sun and wind 35°, wet bulb 32°. The ice consisted of thin transparent scales, lodged in large quantities among the plants; some of these plants, attached to the bottom by thin slender stems, as they waved and turned about in the current, had gathered spherical masses of loosely compacted scales of ice, many of the spheres being 6 inches beneath the surface; when the stem which served to moor them was broken, they immediately rose to the surface and floated along: this process of breaking the moorings was going on spontaneously to a considerable extent. In the last week of December I again visited this locality, after a sharp frost of only 24 hours' duration; before mid-day large masses of loosely compacted ice, composed of numerous scales and thin surface-formed pieces were floating down; and in the place where the ground-ice was before found, these masses stretched over the whole stream, here 17 feet wide, choking it; so that the new arrivals of fresh masses brought down by the current, descended with the water and passed underneath, rising to the surface after the barrier was cleared. I was anxious to examine the plants near this place, but the depth and dark colour of the water rendered it impossible to see any of them. Farther up the stream there was ground-ice in a few favourable places. Temperature of water 32·2°; air in sun and wind 35°; wet bulb 31·5°; mud of bed of stream 34°. Before 3 P.M. most of the floating masses of ice had passed down, and the surface of the stream became remarkably open, the choked part was clear for half its width, and I was surprised to find, that on the plants where a little before I had seen the largest portion of ground-ice there was now none. Shortly after three o'clock, the sun becoming obscured by a horizon cloud, and the wind dying quite away, the temperature of the air sunk rapidly to 25·5°, where it was not stationary 15′. I suppose this severe cold caused a thin veil of cloud on the ground, which checked radiation, for the thermometer rose to near 30°, and continued steady so long as day light sufficed to read it.

On examining some of the ice fished up from the bed of the stream, I found it composed of clusters of scales frozen together as if radiating from a centre; searching the banks, I found masses of similar clusters which appeared to have been formed in the following manner. From late rains the water stood high; at the beginning of the frost, a pellicle of ice was formed along the edges adhering to them. In so flat a district the frost appears rapidly to diminish the supply of water, and the surface of the stream had gradually lowered, leaving the pellicle attached to the banks with an intervening space filled up with clusters of scales; this process appeared to be as efficient for ice-making as the somewhat similar one described as seen on the Pentland Hills. These masses on the banks, no doubt assisted by the power of a bright sun, broke off and fell into the stream, where they formed a part of the floating ice seen passing down: being loosely compacted together, they had often considerable thickness. At the time it appeared to me that, as these passed down they had furnished from their under surfaces, the ground-ice found among the plants; and when they ceased to pass, the ground-ice quickly disappeared.

The frost slackened a little next day without thawing a thin covering of hoar-frost in shaded places. The succeeding night was a moderate frost; and within forty-eight hours from the time I had seen the stream choked with soft masses of floating-ice, I revisited it, when there was little floating, and no ground-ice; although in the stagnant waters the ice coating had increased, all the moving waters had cleared themselves. Temperatures, water of stream 33°, mud of bed at the surface 35°, 6 inches deep 37°, air with a bitter frozen fog 30°.*

The quantities of scales of ice found on the banks of a stream, in many places ready to drop in, through a severe night's frost following a period of wet weather, is a fact well worthy of attention. To me it at once accounts for the great masses of soft ice, Mr Esdaile describes as coming from far up the country to Perth, being a characteristic of the commencement of a severe frost in the river Tay. This floating-ice he also describes as true ground-ice; but I believe it would be more correct to hold that it is one of the sources from which this kind of ice is derived. For other supplies of ice—crystals to streams, there is the same nuclei which exist in stagnant pools; also the drift by the wind of snow or hoar-frost, and the ice-pillars I have already noticed.

The ground-ice I have seen has been described as consisting of thin scales. Many of the published accounts represent the texture as spiculæ like the component parts of snow; and this is generally Mr Elliot's mode of characterising it for the Teviot. The velocity of the current of the stream probably regulates these differences; while, from the observations above given, changes in the same locality may be expected from the kind of weather which has immediately preceded the time of the ground-ice. Dr Davy's† description for the lake district, states the ice to be fine-grained, but he has noticed solid ice in cakes, and on a few stones, under water, these are quite exceptions to the general form of ground-ice, and may admit of explanation through the force of the current transporting them from the place of their formation at the surface.

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* During this fog, I remarked that a thermometer bulb warmed by the hand and then exposed, got much sooner coated with ice, than when exposed cold and dry.

† Edinburgh Philosophical Journal, vol. lxxvii., p. 7.

If we now suppose that some loose small pieces of ice have got entangled in the plants in the bed of a stream, and around these as a nucleus water continued to freeze, what should be the texture of such ice?

I have observed in rapid parts of small streams, where stout twigs of decayed hawthorn touch the current, clear nodules of solid ice are often found; while, when an elastic willow branch touches the water, it collects a mass of white ice, through its continual plunging and rising. When a thin sided vessel of water is suspended in a frosty atmosphere, ice first forms on the surface, and then stretches in shoots of clear hard ice down the sides, and if the vessel is large the ice forms a model of its interior surface. When the upper surface of the same vessel is thickly covered over with a good non-conductor of heat, then hard solid ice is formed under water on the thin exposed part; again, with the same arrangements, if the bottom be made porous a considerable mass of bottom ice is then obtained, which, in every case I have tried, was solid. If the vessel be immersed in a freezing mixture of salt and snow, hard white ice is produced with unboiled water, and with water well freed from air, hard opalescent ice due to a fine grained crystal, with the portions, frozen after the severity of the cold had relaxed, very transparent. All these facts tend to prove that ice formed in the bed of a stream where no air reaches, and where the rate of freezing can never be rapid, should be solid like the nodules on unyielding nuclei at rapids on the surface, or like the ice formed in the bottom of a porous vessel.

In examining this question, the most difficult part to determine, is the freezing together of scales after they have been entangled in the bed of the stream. Had the weather, at the time I found frozen clusters of scales on the plants, at all favoured the supposition that they had frozen together under water, I would have returned filled with doubts and hesitations as to the truth of the entangling hypothesis, but the previous weather rendered this impossible. The explanation I have given of the submergence of these clusters will, I believe, be found to hold good for other localities; when the general explanation of the phenomenon of ground-ice for the climate of Great Britain, will become simply this:—*That it is all, either formed on, or thrown in on, the surface, and made to assume an unnatural position by the mechanical action of the current.*

On this question—Is there identity between the Species of the Secondary and Tertiary Formations, and those belonging to the Existing Generations?

We readily learn, by comparing the ancient generations with those that are contemporary with ourselves, that they have nothing in common with regard to their specific type, although it is not always thus with the generic type. But in order to comprehend the whole value of these differences, it is necessary to enter into some details on this point.

The greater part of the species of geological times differ more or less from the existing generations, and cannot be confounded with them. In the opinion of many palæontologists, among whom it is sufficient to name MM. Agassiz and Deshayes, there is perhaps no species identical between these two creations. MM. F. J. Pictet and D'Orbigny have also adopted the same opinion in their palæontological writings.

Although the general diversity of the beings of the ancient generations from those that compose the new, is not altogether demonstrated, it appears at all events so much the more striking, as we compare species belonging to the most opposite ages. It is particularly among the species of the most remote times, that the dissimilarity to the living races is most conspicuous. That these generations have fewer points of analogy is probably owing to this, that there is little parity between the climateric circumstances and exterior media to whose influence they were subjected.

Accordingly, organic species present similar characters only where the climateric conditions (comprehending in these conditions influences of every kind, which exert some action on their life) are nearly the same. We can thence conceive why we discover fossil species approaching the living races only among the last geological periods. But is it certain that these species are really identical with the latter? We must examine into this matter.

In the earliest times of the study of fossils, a multitude of living species were associated with them, owing to the want of strict examination, and also, perhaps, in consequence of mental indolence, which disposes us to simplify the objects of our study. More attentive comparisons have proved to us, that there may not exist a single species of the secondary formations like the presently existing races. They have even led us to presume, that such also is the case with those belonging to the most recent epoch of the tertiary formations.

As an example of these facts, we may remind our readers, that M. Agassiz, to whom we are indebted for such beautiful researches on fossil fishes, has not found among those of the secondary series more than a single species which can be connected with the races of our own day.

This exception is limited to such an insignificant number, that it is probably an exception only in appearance. How many characters have disappeared in the incomplete remains from which we are obliged to reconstruct the beings of the ancient world! If the animals of ages so remote from us, considered as identical, even after a minute comparison, were to return to life, a multitude of distinctions, of which we had not the least idea, would appear, and shew us how much we erred respecting their pretended affinities.

Yet the complete difference between the animals of our epoch and those of the secondary and tertiary periods, has not been admitted by all naturalists. It has been denied by M. Ehrenberg, who has found a perfect analogy between many infusoria of the cretaceous formations, and those still alive.

If it is possible to be deceived in the examination of animals as complicated as the vertebrata, how much more easy is it to fall into error when attempting to determine infusoria, whose diameter scarcely equals the 300th of a millimetre, and whose organization is remarkable for its simplicity!

M. Ehrenberg himself has given us a proof of the difficulty of determining these infinitely minute beings. He has proved that twelve species of vorticella were only different states of a thirteenth species. Yet of these, a great number of different groups had been formed, and the genera *Eulissa*, *Redella*, *Kerobalana*, *Craterina*, *Ophridia*, and *Urceolaria*, established upon them.

On the other hand, Mr Owen, by considering the general characters of the present population of Australia, is surprised that that of the later geological times could present us with animals of the genera of the Mastodons. In fact, there has not hitherto been found there any living pachyderm, either fossil or (*humatile ?*) Yet the remains of a terrestrial mammifer very analogous, in its teeth, to one of the species of Mastodon common in America and Europe, have been found in the ossiferous grottoes of that country.

The discovery of such an animal, which has no analogy to the mammifera actually living on the continent of New Holland, has led Mr Owen to suppose, that this Mastodon might belong to an entirely different family. He has, therefore, referred this great *humatile* quadruped to a group of Marsupials or Didelphous Mammifera, whose principal types represent, in this continent, the parallel types of the different families of the great classes of mammifera.

Mr Owen has not admitted this supposition without doubt;* but it does not the less prove how difficult it is to be certain, not only of the exact determination of a race belonging to geological times, but even of the family or class to which it belonged.

These difficulties are so much greater, because it is far from being settled what we ought to understand by a species, especially when we wish to verify experimentally, whether generation confirm, or otherwise, the distinctions which have led us to consider them as different, or the analogies by which we seek to assimilate them. Do the marine species undergo gradual modifications by a change in the degree of saltness of the waters in which they live, or in consequence of any other exterior circumstance? Who is unacquainted with the effects these causes produce on the mollusca, and, for example, on many species of *Murex*, *Pterocera*, *Buccinum*, *Oliva*, and particularly on the *Cardium edule?*

Thus, according to Mr Gray, the shells of *Buccinum undatum*, and of *Buccinum striatum* of Pennant, differ only in this, that the former, formed in agitated waters, becomes thick and heavy. The second, which has lived in the tranquil waters of harbours, has there become light, smooth, and often coloured.

The shells which are provided with branched or dilated prominences, such as *Murex*, are likewise subject to great changes, according to the circumstances in which they happen to be placed. Many varieties, produced by local causes, have been improperly considered as distinct species.

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* New researches, and more complete bones, have shewn, that in reality this animal was not a Mastodon, and that it ought to occupy a place in the sub-class of Marsupials.

The *Murex angulifer* is only the *Murex ramosus* with simple prominences; and the *Murex erinaceus*, *subcarinatus*, *anguliferus*, *tarentinus* and *polypterus*, are varieties of the same species. In like manner, the *Murex magellanicus* of tranquil waters appears covered with large sharp foliaceous expansions, while, in agitated seas, the same shell has no such expansions, and has only the ribs crossed. This species rarely attains a large size; when it does become large, it is solid, and loses almost all the appearance of striæ.

The modifiations which shells undergo in consequence of the diversity of their stations, are so much the more important to be noticed, since these calcareous pieces are almost the only remains of the mollusca of the ancient world that have come down to us. Similar effects are not less sensible on fresh water molluscs. *Lymnæus pereger* furnishes us with a remarkable example of such changes. When the rivers of Sweden, and probably of other places, carry it to the Baltic, it is so modified by the different nature of the waters in which it is successively plunged, that it cannot be recognised. To such a degree is this carried, that it has been regarded as another species, and another specific name assigned to it.

Similar modifications have likewise taken place in other species of the same genus, in *Lymnæus ovatus* for example, and that from causes less powerful and less active than those whose effects are so obvious on *Lymnæus pereger*. It would undoubtedly happen that, if we could not observe in what manner these modifications are brought about, if we could not follow their gradations, and observe the transitions of these shells into one another, we would create them into arbitrary species, since they would not be founded on the nature of things.

We may remark, in relation to shells, that there are multitudes of them whose characters we appreciate only by the assistance of the *moules*, whether interior or exterior, from which we may judge of the degree of uncertainty we must be in when we proceed on this single means of determination. In fact, these *moules* undergo numerous variations, according to their degree of preservation; it may even happen, from a series of accidental circumstances, that they present characters which do not really belong to them. May we not suppose that, if we were acquainted with the numerous modifications of the domestic races only from parts of their skeleton, which had been left in the strata of the earth as fossils, we would form many distinct species out of one and the same race? The characters which led us to separate them, would acquire in our eyes an importance as great as those which now serve to distinguish the ancient generations from the existing creations.

There exists, therefore, at least uncertainty in the determination of a great number of fossil species, when we consider them as distinct, when we regard them as having no representatives in nature, and finally when we assimilate them to the races of our own epoch. It is more especially in making these approximations that we run most risk of deceiving ourselves; for here all control is impossible. On the other hand, a multitude of characters are wanting to enable us to judge of the identity of races which have belonged to periods so different in the whole of their conditions and their exterior media.

This identity, whether it be real or not, cannot well be discerned, except in regard to geological eras most nearly approaching the period to which we belong.* In fact, it is only in the most recent tertiary strata that species have been observed, which present analogies to our own, sufficiently striking to admit of identifying them. These affinities between the old and presently existing generations do not, therefore, exist, but in regard to such races as have been subjected to the same effects, and experienced nearly similar influences.

The destruction of the species of the ancient world, seems therefore to have been produced by a change of the exterior circumstances in the media in which they lived. When these changes in the conditions of the media have been considerable and sudden, they have exterminated the species, which were not sufficiently strong and vivacious to withstand these new impressions.

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* It was supposed that species of our own epoch, such as *Murex trunculus* and *Ostræa hippopus*, had been met with in the tertiary formations of Belnay, near Tournus (Saone and Loire); but M. Virlet, who has examined these places, is convinced that these shells, the animals of which had been used as food, had been buried there by the hand of man. All idea of the marvellous is thus banished. (See *Archives*, 1846, t. i., p. 338.)

The vigorous species have alone been able to pass from geological to historical times, if it be indeed the case that species exist common to these two periods. We have already said, that recent observations tend to prove the contrary. If the fact of the survival of certain species, and the extinction of others, had been extricated from the uncertainty which still envelopes it, a remarkable agreement would prevail between the physical data and the account given in the Bible. We would no longer have to ask why one portion of organized beings have been destroyed, in the different phases which the ancient creations have gone through, while others have been spared during the modifications of the surface of the globe, and have passed from geological to historical times.

The partial destruction of certain species of the ancient world, and the survival of others belonging to the same period, cannot well accord with the best established geological facts. According to these facts, there has been a succession in the appearance of living beings; but this succession has taken place in such a manner, that these beings have differed from each other, not only from one period to another, but from one period to that which preceded or followed it. Now these epochs, admitted solely for the purpose of rendering the study of the ancient generations more easy, are less real and distinct than such as exist between the creations of geological times and those of the present period. It is therefore probable, according to the general tenor of the best established facts, that the two grand phases through which the earth has passed, have witnessed the appearance of creations totally different, and having nothing in common between them.

The phenomenon of generations buried in the strata of the earth gives rise to yet other difficulties; we shall direct attention to these, although it may be only to leave the argument without a reply.

We may consider as belonging to the existing periods, these organised beings whose remains are found in the midst of diluvian deposits, and that with so much the more reason, because these deposits likewise exhibit some traces of the human species. Among a multitude of animal remains, which have now their representatives in nature, there is a pretty considerable number of which no vestige can be observed on the surface of the earth. It may be asked how, and by what circumstances, have these latter been destroyed, while the others composing the generations with which the earth is covered, have survived the causes by which the former have perished.

These difficulties are greater still to those who keep in mind the account in the Bible, according to which "animals clean and unclean, fowls of the air, and every thing that moveth on the earth, entered into the ark," and were thereby preserved, according to the will of God. In order to make this text agree with the facts, we may consider it probable that, by the expression, *all that moveth upon the earth*, the sacred writer meant only the principal animals, and not the totality of the species. We have already indicated other passages when the word *all* expresses only a part or portion of the object which Genesis has thus generalized. If we adopt this interpretation, admitted by a great number of Bible commentators, all difficulties vanish, and the facts explain themselves.

The lost species would thus be those which had been destroyed by the deluge, as mentioned in the 21st, 22d, and 23d verses of the eighth chapter of Genesis. There is no need of very extraordinary causes to produce the destruction of a great number of living species, since there are many belonging to the existing creations, and of which we now find no traces on the earth, although certain of them had been perceived in times not very distant from our own.

Such is the case with the Dodo, which was seen in 1616, in the island of Mauritius and in Bourbon, and of which some remains exist in the museum of London, Oxford, and Leyden.* We no longer find this bird; and since that time it has not been observed elsewhere. The race is now, therefore, extinct, as appears to be the case with that of the gigantic horned stag.

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* It even appears that the Paris Museum contains some remains of it.

Yet this ruminant was represented in the pictures of ancient Rome, and, according to the writers of antiquity, it was sent from England on account of the delicacy and excellence of its flesh. The same animal was known to Oppian, Jonston, Aldrovandus, and Munster, who appear to have seen it alive; the latter even pretends that he ate of it, which would prove that the species was not extinct in 1550.

The bone of a gigantic stag, presenting a callosity produced by a pointed and cutting instrument, and found by Mr Hart in the Vale of Arno in Italy, confirms the report of these writers, or, at least, demonstrates that the destruction of this animal has taken place posterior to historical times.

The Dinornes are likewise birds of which we know no representatives now living. They belong, however, to our epoch, for their nests have been discovered on the coasts of New Holland. These nests are as remarkable for their dimensions as the birds which construct them. These animals evidently belong to the existing period, for the natives of New Zealand have preserved the recollection of them, and name them *Moa*.

It is even possible that the ancients may have represented the Dinornes on their monuments, and that these birds are no other than the gigantic cranes mentioned in the legends of eastern nations. M. Bonomi saw, upon the tomb of an officer of Pharaoh belonging to the fourth dynasty, a bas-relief on which birds of this kind were represented, the dimensions of which were very large.*

No doubt the disappearance of those species which lived not long since, may appear at first sight very extraordinary, but it is easily explained when we attend to the organization of these animals. Thus the Dodo, for example, which was formed neither for running nor flight, could not escape from our pursuit. The size of the gigantic stag prevented it, in like manner, evading the attacks which were made upon it. The establishment of Europeans in the island of Mauritius, has thus been the principal cause of the disappearance of the Dodo, just as the clearing away of the forests which covered the surface of ancient Germany has occasioned the loss of one of the most remarkable animals of our world.

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* See Bibl. Univ., 1845, vol. lviii., p. 395.

Many of the animals figured or sculptured on the monuments of antiquity, and uniting conditions which render their existence possible, have no longer representatives, as is also the case with certain species buried in the ancient catacombs of Egypt. Geoffroy Saint Hilaire has sought in vain in that country for traces of the two crocodiles which he found embalmed in the Egyptian tombs. These two races, named by him *Crocodilus lacunosus* and *Crocodilus complanatus*, are still to be sought for among the living races.

There is no need, therefore, of extraordinary causes in order to destroy certain species; it is sufficient to bring about this result, that the species cannot escape our pursuits, nor defend themselves against our attacks. It is obvious that they become extinct, so much the more speedily if their fecundity be inconsiderable, and the number of deaths exceeds that of births. The loss of a species may, therefore, depend on the simplest circumstances, and be the effect of causes by no means beyond the ordinary course of things.

When we study, as a whole, the organized beings which in their turn have animated the world, which, from the subject of our investigations, we are struck with the permanency of the generic types. A pretty considerable number of these types, often very natural, have run through all the ages with the same characters, and have undergone alterations only in their specific distinctions. The species of these persistent genera have only differed from one epoch to another, and most of all from the ancient creations to the present ones. There are, however, many races of our own times which have preserved the same characters which distinguish those of the first ages.

The greatest variations, then, have taken place, not in the generic type, but in the specific type which characterizes, and forms the difference between organized beings, all the individuals of which it embraces. It is particularly among the less complicated beings in the vegetable and animal kingdom that this persistence is most evident, and extends to a very great number of generic sections.

We have considered a species as a type easy to seize, and the determination of which can give rise to no uncertainty. This, however, is far from being the case. In truth, the specific characters, even among living species which we may be said to have continually under our eye, are essentially variable, and it is often difficult to appreciate their true value.

If, then, we submit the vegetable and animal species, admitted into our catalogues, to a strict and complete revision, we may affirm, without fear of error, that the number would be singularly reduced. If we do the same with regard to the species of the old creations which have been considered identical with the existing races, we will perceive how little foundation there is for the analogies by means of which they are associated. This truth would become more evident still if the fossil species presented themselves to us with the whole of their characters unimpaired, for we would then perceive differences of which we can form no idea in the imperfect state of their preservation.

When we examine the whole of the ancient creations, and consider how imperfect are the remains by means of which we endeavour to attain to their specific determinations, we are rather disposed to regard these determinations as provisionary than as expressing absolute certainty. Probably the analogies, by aid of which endeavours have been made to approximate certain species of the ancient world to those of the present, are not so real as they have been supposed; at least they do not rest on the positive knowledge of their characters, since the greater part of these have disappeared in the fossil races.

Accordingly, in consequence of the disappearance of these specific characters, certain ancient species have been assimilated to the living races, an association which certainly would never have been made if the former had been known to us in their perfect state. The greater part of those respecting which we possess certain data, prove that there is nothing in common between the two creations. The difference between the races of the two worlds, is so much the greater as they belong to the most ancient ages; but, independently of this circumstance, which always leads to the same result, there is nothing identical between the generations which belong to epochs so diverse. Geological facts support, with all their weight, this remarkable circumstance, which an enlightened and attentive comparison of the species of the two creations confirms more and more every day.

The races belonging to the ancient creations have, therefore, no longer any representatives on the earth, but, what is no less remarkable, it is the same with a portion of the actual races. Many species of the creation to which we belong have perished; some of them at a period of a cataclysm which has desolated a great part of the earth's surface, and others in recent times.

These facts, singular and extraordinary as they may appear at first sight, are yet true in regard to a certain number of the species of different epochs, though they belong to our period. When we reflect on the causes which have destroyed many races of our creation, we are soon convinced that they are very simple, and that the fact of their destruction comes within the ordinary course of things. It is enough to produce it, that the number of deaths should be proportionally greater than that of births; the more the former exceeds the latter, the more certain and rapid is the extinction of every race.—(*Marcel de Serres, Bib. Univ., p.* 241, *No.* 7, *Aout* 1846.)

On the Destruction and partial Reproduction of Forests in British North America. By JOHN WILLIAM DAWSON, Esq. of Pictou. Communicated by the Author.

The changes produced by the agency of civilized man, in the condition of the earth's surface, and the numbers and distribution of its living inhabitants, though not of great importance when compared with those which result from the unceasing operation of natural causes, are interesting to the naturalist, as they illustrate the vicissitudes which many parts of the earth's surface have experienced in ancient times, the extent to which plants and animals can accommodate themselves to changes of circumstances, and the natural compensations which have been provided for the destruction or diminution of particular species. Inquiry into such changes is also of importance as a means of dispelling the mystery which frequently envelopes the succession of organised beings in circumstances of physical change; a mystery which has induced some naturalists to recur to the doctrines of spontaneous generation and the transmutation of species, for explanations of phenomena which, if properly examined, would have been found to result from some of the most ordinary causes of the maintenance and distribution of animal and vegetable life.

In North America, and especially in those parts of it forming the United States and British Provinces, such changes have occurred with great rapidity, converting, in a few years, uninhabited forests into countries having the aspect of regions long inhabited by civilized men. The forests have been destroyed, their living inhabitants extirpated or obliged to adopt new modes of life, new animals and plants introduced and naturalised; and, indeed, a revolution effected in all the departments of organised nature, in the lapse of a single generation. To notice a few of these changes, with reference more especially to the destruction and partial reproduction of forests, is my present object. The facts which I propose to state have been collected principally in the province of Nova Scotia.

In their natural state, Nova Scotia and the neighbouring provinces were covered with dense woods, extending from the shores to the summits of the hills. These woods did not form detached groves, but constituted a nearly continuous sheet of foliage, the individual trees composing which were so closely placed as to prevent them from assuming full and rounded forms, and to oblige them to assume tall and slender shapes, that each might obtain air and light. The only exceptions to this are certain rich and usually light soils, where the forest is sometimes more open, and hills too rocky to support a covering of trees. When viewed from the summit of a hill, the forest presents a continuous undulating surface of a more or less dark colour and uneven form, in proportion to the prevalence of the deep colours and uneven outlines of the evergreen coniferæ, or of the lighter tints and rounded contours of the deciduous trees; and these two classes are usually arranged in belts or irregular patches, containing mixtures of trees corresponding to the fertility and dryness of the soil. In general the deciduous or hardwood trees prevail on intervale ground, fertile uplands, and the flanks and summits of slaty and trappean hills; while swamps, the less fertile and lightest upland soils, and granitic hills, are chiefly occupied by coniferous trees.

The forest trees spring from a bed of black vegetable mould, whose surface is rendered uneven by the little hillocks of earth and stones thrown up by windfalls; and which, though usually named *Cradle hills,* are in reality the graves of departed members of the forest, whose trunks have mouldered into the mossy soil. These cradle hills are most numerous in thin soils; and are chiefly produced by the coniferous trees, and especially by the hemlock spruce. There is usually little underwood in the original forest; mosses, lycopodia, ferns, and a few herbaceous flowering plants, however, flourish beneath the shade of the woods.

The woods perish by the axe and by fire, either purposely applied for their destruction or accidental. Forest fires have not been confined to the period of European occupation. The traditions of the Indians tell of extensive ancient conflagrations; and it is believed that some of the aboriginal names of places in Nova Scotia originated in these events. In later times, however, fires have been more numerous and destructive. In clearing land, the trees when cut down are always burned, and, that this may be effected as completely as possible, the driest weather is frequently selected; although the fire then is much more likely to spread into the surrounding woods. It frequently happens that the woods contain large quantities of dry branches and tops of trees, left by cutters of timber and firewood, who rarely consider any part of the tree except the trunk worthy of their attention. Even with out this preparation, however, the woods may in dry weather be easily inflamed; for although the trunks and foliage of growing trees are not very combustible, the mossy vegetable soil, much resembling peat, burns easily and rapidly. Upon this mossy soil depends, in a great measure, the propagation of fires, the only exception being when the burning of groves of the resinous coniferous trees is assisted by winds, causing the flame to stream through their tops, more rapidly than it can pass along the ground. In such cases some of the grandest appearances ever shewn by forest fires, occur. The fire, spreading for a time along the ground, suddenly rushes up the tall resinous trees with a loud crashing report, and streams far beyond their summits, in columns and streamers of lurid flame. It frequently happens, however, that in wet or swampy ground, where the fire cannot spread around their roots, even the resinous trees refuse to burn; and thus swampy tracts are comparatively secure from fire. In addition to the causes of the progress of fires above referred to, it is probable that at a certain stage of the growth of forests, when the trees have attained to great ages, and are beginning to decay, they are more readily destroyed by accidental conflagrations. In this condition the trees are often much moss-grown, and have much dead and dry wood; and it is possible that we should regard fires arising from natural or accidental causes, as the ordinary and natural agents for the removal of such worn-out forests.

Where circumstances are favourable to their progress, forest fires may extend over great areas. The great fire which occurred in 1825, in the neighbourhood of the Miramichi river, in New Brunswick, devastated a region 100 miles in length and 50 miles in breadth. One hundred and sixty persons, and more than 800 cattle, besides innumerable wild animals, are said to have perished in this conflagration. In this case, a remarkably dry summer, a light soil easily affected by drought, and a forest composed of full-grown pine trees, concurred, with other causes, in producing a conflagration of unusual extent.

When the fire has passed through a portion of forest, if this consist principally of hardwood trees, they are usually merely scorched,—to such a degree, however, as in most cases to cause their death; some trees, such as the birches, probably from the more inflammable nature of their outer bark, being more easily killed than others. Where the woods consist of softwood or coniferous trees, the fire often leaves nothing but bare trunks and branches, or at most a little foliage, scorched to a rusty-brown colour. In either case, a vast quantity of wood remains unconsumed, and soon becomes sufficiently dry to furnish food for a new conflagration; so that the same portion of forest is liable to be repeatedly burned, until it becomes a bare and desolate "barren," with only a few charred and wasted trunks towering above the blackened surface. This has been the fate of large districts in Nova Scotia and the neighbouring colonies; and as these burned tracts could not be immediately occupied for agricultural purposes, and are diminished in value by the loss of their timber, they have been left to the unaided efforts of nature to restore their original verdure. Before proceeding to consider more particularly the mode in which this restoration is effected, and the appearances by which it is accompanied, I may quote, from an article in a colonial periodical, the views of Mr Titus Smith, secretary of the Board of Agriculture of Nova Scotia, on this subject. These views, as the results of long and careful observation, are entitled to much respect.

"If an acre or two be cut down in the midst of a forest, and then neglected, it will soon be occupied by a growth similar to that which was cut down; but when all the timber, on tracts of great size, is killed by fires, except certain parts of swamps, a very different growth springs up; at first a great number of herbs and shrubs, which did not grow on the land when covered by living wood. The turfy coat, filled with the decaying fibres of the roots of the trees and plants of the forest, now all killed by the fire, becomes a kind of hot-bed, and seeds which had lain dormant for centuries, spring up and flourish in the mellow soil. On the most barren portions, the blueberry appears almost everywhere; great fields of red raspberries and fire-weed or French willow, spring up along the edges of the beech and hemlock land, and abundance of redberried-elder and wild redcherry appear soon after; but in a few years, the raspberries and most of the herbage disappear, and are followed by a growth of firs, white and yellow birch, and poplar. When a succession of fires has occurred, small shrubs occupy the barren, the kalmia or sheep-poison being the most abundant; and, in the course of ten or twelve years form so much turf, that a thicket of small alder begins to grow, under the shelter of which fir, spruce, hacmetac (larch), and white birch spring up. When the ground is thoroughly shaded by a thicket 20 feet high, the species which originally occupied the ground begins to prevail, and suffocate the wood which sheltered it; and within sixty years, the land will generally be covered with a young growth of the same kind that it produced of old." Assuming the above statements to be a correct summary of the principal modes in which forests are reproduced, we may proceed to consider them more in detail.

1*st*, Where the wood is merely cut down and not burned, the same description of wood is immediately reproduced, and this may be easily accounted for. The soil contains abundance of the seeds of these trees, there are even numerous young plants ready to take the place of those which have been destroyed; and if the trees have been cut in winter, their stumps produce young shoots. Even in cases of this kind, however, a number of shrubs and herbaceous plants, not formerly growing in the place, spring up; the cause of this may be more properly noticed when describing cases of another kind. This simplest mode of the destruction of the forest, may assume another aspect. If the original wood have been of kinds requiring a fertile soil, such as maple or beech, and if this wood be removed, for example, for firewood, it may happen that the quantity of inorganic matter thus removed from the soil may incapacitate it, at least for a long time, from producing the same description of timber. In this case, some species requiring a less fertile soil may occupy the ground. For this reason, forests of beech growing on light soils, when removed for firewood, are sometimes succeeded by spruce and fir. I have observed instances of this kind, both in Nova Scotia and Prince Edward Island.

2*dly*, When the trees are burned, without the destruction of the whole of the vegetable soil, the woods are reproduced by a more complicated process, which may occupy a number of years. In its first stage, the burned ground bears a luxuriant crop of herbs and shurbs, which, if it be fertile and not of very great extent, may nearly cover its surface in the summer succeeding the fire. This first growth may comprise a considerable variety of species, which we may divide into three groups. The first of these consists of herbaceous plants, which have their roots so deeply buried in the soil as to escape the effects of the fire. Of this kind, is a small species of Trillium, whose tubers are deeply imbedded in the black mould of the woods, and whose flowers may sometimes be seen thickly sprinked over the black surface of woodland very recently burned. Some species of ferns, also, in this way, occasionally survive forest fires. A second group is composed of plants whose seeds are readily transported by the wind. Of this kind, is the species of Epilobium, known in Nova Scotia as the fire-weed or French willow, whose feathered seeds are admirably adapted for flying to great distances, and which often covers large tracts of burned ground so completely, that its purple flowers communicate their own colour to the whole surface, when viewed from a distance. This plant appears to prefer the less fertile soils, and the name of fire-weed has been given to it, in consequence of its occupying these when their wood has been destroyed by fire. Various species of Solidago and Aster, and other composite plants, and Ferns, Lycopodia, and Mosses, are also among the first occupants of burned ground, and their presence may be explained in the same way with that of the Epilobium; their seeds and sporules being easily scattered over the surface of the barren by wind. A third group of species, found abundantly on burned ground, consists of plants bearing edible fruits. The seeds of these are scattered over the barren by birds which feed on the fruits, and finding a rich and congenial soil, soon bear abundantly and attract more birds, bringing with them the seeds of other species. In this way, it sometimes happens that a patch of burned ground, only a few acres in extent, may, in a few years, contain specimens of nearly all the fruit-bearing shrubs and herbs indigenous in the country. Among the most common plants which overspread the burned ground in this manner, are the raspberry, which, in good soils, is one of the first to make its appearance; two species of vaccinium, called in Nova Scotia, blueberries; the tea-berry or wintergreen *(Gaultheria procumbens)*; the pigeon-berry (*Cornus canadensis*); and the wild strawberry. It is not denied that some plants may be found in recently burned districts, whose presence may not be explicable in the above modes; but no person acquainted with the facts, can deny that all the plants which appear, in any considerable quantity, within a few years after the occurrence of a fire, may readily be included in the groups which have been mentioned. By the simple means which have been described, a clothing of vegetation is speedily furnished to the burned district; the unsightliness of its appearance is thus removed, abundant supplies of food are furnished to a great variety of animals, and the fertility of the soil is preserved, until a new forest has time to overspread it.

With the smaller plants which first cover a burned district, great numbers of seedling trees spring up, and these, though for a few years not very conspicuous, eventually overtop, and, if numerous, suffocate the humbler vegetation. Many of these young trees are of the species which composed the original wood, but the majority are usually different from the fomer occupants of the soil. The original forest may have consisted of white or red pine; black, white, or hemlock spruce; maple, beech, black or yellow birch, or of other trees of large dimensions, and capable of attaining to a great age. The "second growth" which succeeds these, usually consists of poplar, white or poplar birch, wild cherry, balsam fir, scrub pine, alder, and other trees of small stature, and usually of rapid growth, which, in good soils, prepare the way for the larger forest trees, and occupy permanently, only the less fertile soils. A few examples will shew the contrast which thus appears between the primeval forest and that which succeeds it after a fire. Near the town of Pictou, woods chiefly consisting of beech, maple and hemlock, have been succeeded by white birch and firs. A small clearing in woods of maple and beech in New Annan, which, thirty years ago, was under cultivation, is now thickly covered with poplars thirty feet in height. In Prince Edward Island, fine hardwood forests have been succeeded by fir and spruce. The pine woods of Miramichi, destroyed by the great fire above referred to, have been followed by a second growth, principally composed of white birch, poplar, and wild cherry. When I visited this place, a few years since, the second growth had attained to nearly half the height of the dead trunks of the ancient pines, which were still standing in great numbers.

As already stated, the second growth almost always includes many trees similar to those which preceded it, and when the smaller trees have attained their full height, these, and other trees capable of attaining a greater magnitude, overtop them, and finally cause their death. The forest has then attained its last stage, that of perfect renovation. The cause of the last part of the process evidently is, that in an old forest, trees of the largest size and longest life have a tendency to prevail, to the exclusion of others. For reasons which will be afterwards stated, this last stage is rarely attained by the burned forests, in countries beginning to be occupied by civilized man.

In accounting for the presence of the seeds necessary for the production of the second growth, we may refer to the same causes which supply the seeds of the smaller plants appearing immediately after the fire. The seeds of many forest trees, especially the poplar, the birch, and the firs, and spruces, are furnished with ample means for their conveyance through the air. The cottony pappus of the poplar seems especially to adapt it for this purpose. The seeds of the wild cherry, another species of frequent occurrence in woods of the second growth, is dispersed by birds, which are fond of the fruit; the same remark applies to some other fruit-bearing species of less frequent occurrence. When the seeds that are dispersed in these ways fall in the growing woods, they cannot vegetate, but when they are deposited on the comparatively bare surface of a barren, they readily grow; and if the soil be suited to them, the young plants increase in size with great rapidity.

It is possible however, that the seeds of the trees of the second growth may be already in the soil. It has been already stated, that deeply buried tubers sometimes escape the effects of fire, and, in the same manner, seeds imbedded in the vegetable mould, or buried in cradle hills, may retain their vitality, and being supplied by the ashes which cover the ground, with alkaline solutions well-fitted to promote their vegetation, may spring up before a supply of seed could be furnished from any extraneous source. It is even probable that many of the old forests may already have passed through a rotation similar to that above detailed, and that the seeds deposited by former preparatory growths may retain their vitality, and be called into life by the favourable conditions existing after a fire. This is a point, however, requiring for its establishment a series of experiments which I have not yet been able to undertake.

If, as already suggested, forest fires, in the uncultivated state of the country, be a provision for removing old and decaying forests, then such changes as those above detailed, must have an important use in the economy of nature, since by their means different portions of the country would succeed each other in assuming the state of "barrens," producing abundance of herbs and wild fruits suitable for the sustenance of animals which could not subsist in the old forests; and these gradually becoming wooded, would keep up a succession of young and vigorous forests.

Thirdly, The process of restoration may be interrupted by successive fires. These are most likely to occur soon after the first burning, but may happen at any subsequent stage. The resources of nature are not, however, easily exhausted. When fires pass through young woods, some trees always escape; and so long as any vegetable soil remains, young plants continue to spring up, though not so plentifully as at first. Repeated fires, however, greatly impoverish the soil, since the most valuable part of the ashes is readily removed by rains, and the vegetable mould is entirely consumed. In this case, if the ground be not of great natural fertility, it becomes incapable of supporting a vigorous crop of young trees. It is then permanently occupied by shrubs and herbaceous plants; at least these remain in exclusive possession of the soil for a long period. In this state, the burned ground is usually considered a permanent barren; a name which does not, however, well express its character, for though it may appear bleak and desolate when viewed from a distance, it is a perfect garden of flowering and fruit-bearing plants, and of beautiful mosses and lichens. There are few persons born in the American colonies, who cannot recal the memory of happy youthful days spent in gathering flowers and berries in the burnt barrens. Most of the plants already referred to as appearing soon after fires, continue to grow in these more permanent barrens. In addition to these, however, a great variety of other plants gradually appear, especially the Kalmia angustifolia or sheep laurel, which often becomes the predominant plant over large tracts. Cattle straying into the barrens deposit the seeds of cultivated plants, as the grasses and clovers, as well as of many exotic weeds, which often grow as luxuriantly as any of the native plants.

Lastly, When the ground is permanently occupied for agricultural purposes, the reproduction of the forest is of course entirely prevented. In this case, the greater number of the smaller plants found in the barrens disappear. Some species of the Solidago and Aster, and the Canada thistle, as well as a few smaller plants, remain in the fields, and sometimes become troublesome weeds. The most injurious weeds found in the cultivated ground, are not, however, native plants, but foreign species, which have been introduced with the cultivated grains and grasses; the ox-eyed daisy or white weed, and the crowsfoot or buttercup, are two of the most abundant of these.

When a district has undergone the last change, when the sombre woods and the shade-loving plants that grow beneath them, have given place to open fields, clothed with cultivated plants, the metamorphosis which has taken place extends in its effects to the indigenous animals; and in this department, its effects are nearly as conspicuous and important as in relation to vegetation. Some wild animals are incapable of accommodating themselves to the change of circumstances; others at once adapt themselves to new modes of life, and increase greatly in numbers. It was before stated that the barrens, when clothed with shrubs, young trees, and herbaceous plants, were in a condition highly favourable to the support of wild animals; and perhaps there are few species which could not subsist more easily in a country at least partially in this state. For this reason, the transition of a country from the forest state to that of burned barrens is temporarily favourable to many species, which disappear before the progress of cultivation; and this would be more evident than it is, if European colonization did not tend to produce a more destructive warfare against such species than could be carried on by the Aborigines. The ruffed grouse, a truly woodland bird, becomes, when unmolested, more numerous on the margins of barrens and clearings than in other parts of the woods. The hare multiplies exceedingly in young second growths of birch. The wild pigeon has its favourite resort in the barrens during a great part of the summer. The moose and cariboo, in summer, find better supplies of food in second growth and barrens than in the old forests. The large quantities of decaying wood, left by fires and woodcutters, afford more abundant means of subsistence to the tribe of woodpeckers. Many of the fly-catchers, warblers, thrushes, and sparrows, greatly prefer the barrens to most other places. Carnivorous birds and quadrupeds are found in such places in numbers proportioned to the supplies of food which they afford. The number of instances of this kind might be increased to a great extent if necessary; enough has, however, been stated to illustrate the fact.

Nearly all the animals above noticed, and many others, disappear when the country becomes cultivated. There are, however, other species which increase in numbers, and at once adapt themselves to the new conditions introduced by man. The robin (Turdus migratorius) resorts to and derives its subsistence from the fields, and greatly multiplies, though much persecuted by sportsmen. The Fringilla nivalis, a summer bird in Nova Scotia, becomes very familiar, building in outhouses, and frequenting barns in search of food. The song sparrow and Savannah finch, swarm in the cultivated ground. The yellow bird (Sylvia æstiva) becomes very familiar, often building in gardens. The golden-winged woodpecker resorts to the cultivated fields, picking grubs and worms from the ground. The cliff-swallow exchanges the faces of rocks for the eaves of barns and houses, and the barn and chimney swallows are everywhere ready to avail themselves of the accommodation afforded by buildings. The acadian or little owl makes its abode in barns during winter. The bob-lincoln, the king bird, the waxwing or cherry bird, and the humming bird, are among the species which profit by the progress of cultivation. The larger quadrupeds disappear, but the fox and ermine still prowl about the cultivated grounds, and the field-mouse (Arvicola Pennsylvanica) which is very abundant in some parts of the woods, is equally so in the fields. Many insects are vastly increased in numbers, in consequence of the clearing of the forests. Of this kind are the grasshoppers and locusts, which, in dry seasons, are very destructive to grass and grain; the frog-spittle insects (Cercopis) of which several species are found in the fields and gardens, and are very injurious to vegetation; and the lepidoptera, nearly the whole of which find greater abundance of food and more favourable conditions in the burned barrens and cultivated fields, than in the growing woods. It may be remarked, in general, that there is no animal, frequenting in Europe the cultivated grounds, and either beneficial or noxious to man, which has not, in the indigenous species of America, an exact representative, filling its place in the economy of nature, and often, in a natural historical point of view, closely related to it. This results from the general sameness of arrangement in the system of nature in the old and new world; and if studied in its details, would form a subject of great interest to the zoologist and physical geographer.

On the Tertiaries of the Island of Cos. By Professor EDWARD FORBES. With a Plate of Fossil Shells.

When describing the fresh-water tertiaries of the valley of the Xanthus, we referred to similar tertiaries in the island of Cos. These were visited by us, on our voyage from Rhodes to rejoin the Beacon, after leaving Lycia. They present phenomena of striking interest and importance; and as they bear on the question of the possibility of a transmutation of species, we cannot do better than append a notice of them to this account of the geology of Lycia.

The fresh-water tertiaries of Cos are of considerable extent. They appear to belong to the same geological period with those of the valleys of Xanthus and Cibyra. We have seen that those Lycian fresh-water beds were of date posterior to the miocene marine formations of the same region. Thus we get an antedate; and in Cos we get a distinct afterdate, for there the same beds, or what were probably beds of the same age, form the walls of a tertiary basin of later date. This basin consists of a well-defined series of marine deposits, containing numerous newer pliocene fossils, identical with those of Rhodes and of Sicily. The Cos fresh-water beds, and those of Lycia, may therefore be regarded as older pliocene at latest.

The fossils in the newer pliocene marine formation are extremely numerous, both as to species and individuals. They consist of such testacea as now live in the neighbouring sea, mingled with others extinct, or known only as inhabitants of the Red Sea and Indian Ocean. Many species, too, now very rare in the neighbouring seas, such as *Nassa semistriata*, and *Siliquaria anguina*, are abundant in the fossil state, whilst others now plentiful in the Ægean are very scarce in the tertiaries. Such shells as the *Phorus* and the *Niso* conspicuously represent the extinct forms.

In the fresh-water strata, against which the marine newer pliocene beds abut unconformably, as represented, A and B, Plate III., there are also numerous and well-preserved fossils; shells of *Paludina*, *Neritina*, *Melanopsis*, *Melania*, *Valvata*, *Unio*, *Cyclas*, and *Planorbis*, occur, marking the nature of the deposit. With them, teeth of a *Cyprinus* were found; also leaves and stems of plants; and in the uppermost stratum shells of the common cockle.

The great interest of this formation depends on certain appearances presented by the mollusca of the genera *Paludina*, *Melanopsis*, and *Neritina*, found in great quantities in several parts of it. They occur distributed in distinct horizons, throughout the vertical thickness of the section. These horizons form three series, each of which is characterised by a peculiar form of *Paludina* and of *Neritina*, not present in the other two; and in the two lower horizons, there are two species of *Melanopsis* peculiar to each.

So very different are the several fossils of one zone from those of another, that at first examination, we appear to have before us very distinct and well-marked species; and that each series of horizons was characterised by *Paludinæ*, *Neritinæ*, and *Melanopsides*, peculiar to itself, and representative of those inhabiting the other divisions. This would be a very startling phenomenon to occur within so limited an area. If the successive species be considered distinct, we must regard them as mutually representative, and hold that a succession of creations and extinctions took place, in this probably limited basin, during a brief geological period, or else that a transmutation of species took place.

A careful study of the forms in question, and an inquiry into the modes and capacity of variation of species in the genera to which they belong, and among their allies, have convinced us, however, that these curious changes of form may be accounted for otherwise; and that in the successive *Paludinæ*, *Neritinæ*, &c., we have before us only the same species assuming protean variations. By reference to the figures in Plate III., it will be seen that the *Paludinæ* and *Neritinæ* of the first or lowest zone, have smooth and unwrinkled shells; that those of the second have their shells belted by a strong fold or corrugation; whilst those of the uppermost zone are deeply sulcated, and surrounded by strong spiral ridges.

Such changes of form take place among Littorinæ and Neritinæ, even now, in places where alternations of fresh and salt water affect the mollusca, and in brackish water localities. There are phenomena in the Cos beds, which warrant us in referring the remarkable peculiarities of the fossils therein found to a similar cause. That an influx of salt water changed the character of the basin in which they lived towards the close of its existence, is evident from the presence of the *Cardium edule* in its uppermost part. That some such cause had previously been in action, is probable from the fact, that the pulmoniferous testacea found in this formation are confined to the lowest series of horizons. One of the authors has elsewhere shewn, that no species of mollusc can live for any length of time on the same ground. A change of ground is necessary for its prosperity, otherwise it dies off. But, as the fry of even the most sedentary testacea are active creatures of a different form, organised for swimming, when all the adult animals upon a ground are destroyed, their descendants may survive their destruction, and replace them, providing the ground be sufficiently changed during the interval.

Now these two facts, *first*, of the nature and causes of the variations among such testacea as present such curious changes of form in the Cos fresh-water beds , and, *second*, of the necessity of a change of ground for the well-being of a species, and the manner in which, owing to the nature of the larva, such change may be effected on the same spot, have led us to propose the following solution of the Cos problem.

The lowest series of horizons were deposited in the basin when it was purely a fresh-water one, and in it we found the *Paludinæ*, &c., in their normal condition, associated with ordinary fresh-water mollusca. These latter are killed off by an influx of salt water, sufficient to render the basin slightly brackish. This influx takes place at a time when the molluscs of the uppermost horizon in the lowest series have exhausted their ground, whilst, at the same time, their fry are swimming in the manner of Pteropods through the waters.

The adults are destroyed, but their descendants survive, so affected, however, by the change in the condition of the element as to assume a new form, and develop themselves under the aspect of distinct species. A second revolution of the same kind brings about a third, still more remarkable, and apparently equally sudden, change, and the continued inroads of the sea at length revolutionise the character of the fauna, introduce marine testacea in the place of the fresh-water species, and destroy the latter altogether. Such an explanation is consistent with what we now know of the modes of variation among fresh-water mollusca, and accounts sufficiently for a very remarkable palæontological phenomenon, which, at first glance, appeared to afford strong support to the notion of a transmutation of species in time.—*Travels in Lycia, by Professor Edward Forbes and Lieut. T. A. B. Spratt, R.N.*, vol. ii., p. 199.

Analysis of Pottery made by the Red Indians of North America. By Mr JOHN MACADAM of Glasgow, Laboratory-Assistant to Dr GEORGE WILSON, F.R.S.E., Edinburgh. Communicated by the Royal Scottish Society of Arts.*

The following is the analysis of a specimen of North American pottery sent from the neighbourhood of Peterborough, Canada West, to Dr Wilson, from whom I received it, and in whose laboratory the analysis hereafter detailed was performed.

Large quantities of broken clay vessels are frequently found, being either turned up by the plough in those parts of the country under cultivation, or else found imbedded in mounds of earth in the interior of the forests in the vicinity, which were at one time, and many parts of which are at present, inhabited by the OJIBBEWAY or CHIPPEWA INDIANS. In all probability, the subject of this communication was the manufacture of that tribe.

Before, however, detailing its chemical composition, it will be interesting to know something of its physical characters, the most prominent of which are as follows.

The pottery is of a brownish-black colour, the outer surface being somewhat reddish. It is exceedingly hard and difficult to fracture. The original form of the vessel cannot be traced; it would appear, however, to have been of considerable dimensions, if we may judge from the shape of its few remaining fragments.

The vessel is ornamented around the sides with a design, which has, no doubt, been taken from nature; it somewhat resembles a pinnate leaf.

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* Read and exhibited 25th January 1847.

The surface is also, almost completely covered with marks like scratches crossing each other, forming a kind of network which has evidently been done with the hand, aided by some rude instrument. This ornamental appearance resembles somewhat in style, that which exists upon the pottery occasionally found in the Druidical tumuli of our own country.

There are little distinct colourless particles distributed throughout its mass, in size from about $\frac{1}{50}$th to $\frac{1}{20}$th of an inch in diameter. These, when examined, were found to be pure silica. It would appear, that these had been purposely introduced into the mixture of which the pottery was made, and that they were obtained by the pulverization of quartz, or some other natural variety of silica.

Organic matter is present to a considerable extent, and, from its general diffusion throughout the mass, would seem to have been added in order to increase the adhesiveness of its particles. In the Babylonian and Egyptian sun-burnt pottery, straw was employed for this purpose. The organic matter present in the American earthenware, however, differs from that present in the ancient Asiatic and African bricks, by its being delicately fibrous, though, like the organic matter present in the Babylonian and Egyptian varieties, it is of vegetable origin.

A portion of the pottery was submitted to qualitative chemical analysis. Its constituents were—

Water.
Organic Matter.
Oxides of Iron.
Alumina.
Lime.
Magnesia.
With traces of Potash and Soda.

The respective quantities of these ingredients were then determined in the usual manner, viz. :—

A portion of the powdered material was fused with an alkaline carbonate. The silica was separated from the fused mass by the addition of hydrochloric acid. From the acid solution containing the soluble matters, the sesquioxide of iron and alumina were jointly precipitated by ammonia. The alumina was removed from the sesquioxide by boiling in potash, and afterwards reprecipitated from its state of solution by adding chloride of ammonium.

The lime present was precipitated as oxalate; this was converted into the carbonate by ignition, and estimated as such.

The magnesia was precipitated as the ammoniacal phosphate—

$$(2\,\mathrm{Mg\,O\,NH^4\,O\,PO^5}.)$$

This, after ignition, was collected as the bibasic phosphate ($2\,\mathrm{Mg\,O\,PO^5}$).

The water and organic matter were estimated according to the usual methods.

Results of Analysis.

Water, ...	1·36
Organic Matter, ...	5·92
Silicic Acid, ...	64·80
Sesquioxide of Iron, . . .	14·30
Alumina, ...	9·95
Lime, ...	4·39
Magnesia	1·43
Alkalies (Potash and Soda), . .	Traces.
	102·15

The increase here represented may, to a great extent, be accounted for by the fact, that nearly all the iron exists in the pottery as protoxide; whereas, during the analysis, it was converted into the sesquioxide, and weighed as such.* If the quantity of iron present had been calculated as protoxide (which would be much nearer the truth), its amount would have been 1·46 less than that stated above, which would reduce the amount of gain during the analysis to ·69.

It is probable that this pottery was not made from the use of any one material found native, but that it was manufactured from a mixture of pulverised silica, ferruginous clay, and organic matter; the silicic acid of which, afterwards combined with the lime, magnesia, alumina, and a portion of the iron, forming silicates.

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* It appeared more desirable to collect all the iron present as the sesquioxide, than trust to the accuracy of any of the processes extant, for determining the respective quantity of each oxide when both are present.

It would also appear, from the small quantity of iron existing in the state of sesquioxide, and from the presence of organic matter, that the heat employed for the purpose of drying or baking the pottery was of no high temperature.

If the heat had approached in temperature that of our pottery kilns, the dark green protoxide of iron would have been for the greater part, if not totally, converted into the red sesquioxide; and, moreover, the organic matter would have been dissipated. This effect may be observed in the clay jars, bricks, &c., manufactured in this country. A common wood fire was probably the only source of heat the Ojibbeways had at command; which being in contact only with the outside of the vessel, has confined its chemical action to that part.

LABORATORY, 24 BROWN SQUARE, EDINBURGH,
December 1846.

On the Minhocao of the Goyanese, an enormous species of Lepidosiren.[*] By M. AUGUSTE DE SAINT HILAIRE.

Luiz Antonio da Silva e Souza, whose acquaintance I made during my travels, and to whom we owe the most valuable researches on the history and statistics of Goyaz, says, in speaking of the lake of Padre Aranda, situated in this vast province,[†] that it is inhabited by minhocôes;[‡] then he adds that these monsters—it is thus he expresses himself—dwell in the deepest parts of the lake, and have often drawn horses and horned cattle under the water.[§] The industrious

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[*] This notice is taken from an unpublished work on the province of Goyaz.

[†] The province of Goyaz stretches from nearly 5° 22′ latitude south, to the 22d degree, and is greater than France.

[‡] Plural of Minhocao.

[§] See Memoria Sorbe o descobrimente, etc. da capitania de Goyaz, in the 'Patriota,' 1814.

Pizarro, who is so well acquainted with all that relates to Brazil, mentions nearly the same thing, and points out the lake Feia, which is likewise situated in Goyaz, as also being inhabited by minhocôes.*

I had already heard of these animals several times, and I considered them as fabulous. When the disappearance of horses, mules and cattle, in fording the rivers, was certified by so many persons, it became impossible for me to doubt it altogether.

When I was at the Rio des Piloes, I also heard much of the minhocôes. I was told that there were some in this river, and that at the period when the waters had risen, they had often dragged in horses and mules whilst swimming across the river.

The word *minhocao* is an augmentative of *minhoca*, which, in Portuguese, signifies *earth-worm;* and, indeed, they state that the monster in question absolutely resembles these worms, with this difference, that it has a visible mouth; they also add, that it is black, short, and of enormous size; that it does not rise to the surface of the water, but that it causes animals to disappear by seizing them by the belly.

When, about twenty days after, having left the village and the river of Piloes, I was staying with the Governor of Meiapont, M. Joaquim Alvez de Oliveira, I asked him about these minhocôes: he confirmed what I had already been told, mentioned several recent accidents caused by these animals, and assured me at the same time, from the report of several fishermen, that the minhocao, notwithstanding its very round form, was a true fish, provided with fins.

I at first thought that the minhocao might be the *Gymnotus carapa*, which, according to Pohl,† is found in the Rio Vermelho, which is near to the Rio des Piloes; but it appears from the Austrian writer that this species of fish bears the name of *Terma termi*, in the country; and, moreover, the effects produced by the Gymnoti are, according to Pohl, well known to the mulattoes and negroes, who often felt them, and have nothing in common with what is related of the minhocao. Professor Gervais, to whom I mentioned my doubts, directed my attention to the description which P. L. Bischoff has given of the Lepidosiren;* and, indeed, the little we know of the minhocao agrees well enough with what is said of the rare and singular animal discovered by M. Natterer.

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* Memorias Historicas, &c., vol. ix. p. 332.

† Reise, vol. i. p. 360.

That naturalist found his *Lepidosiren* in some stagnant waters near the Rio da Madeira and of the Amazon. The minhocao is not only said to be in rivers, but also in lakes. It is, without doubt, very far from the lake Feia to the two localities mentioned by the Austrian traveller; but we know that the heats are excessive at Goyaz. La Serra da Paranahyba e do Tocantim, which crosses this province, is one of the most remarkable dividers of the gigantic water-courses of the north of Brazil from those of the south; the Rio des Piloes belongs to the former, as does the Rio da Madeira.

The *Lepidosiren paradoxa* of M. Natterer has actually the form of a worm, like the minhocao. Both have fins; but it is not astonishing that they have not always been recognised in the minhocao, if, as in the *Lepidosiren*, they are, in the animal of the Rio des Piloes, reduced to simple rudiments. "The teeth of the *Lepidosiren*," says Bischoff, "are well fitted for seizing and tearing its prey; and to judge of them from their structure, and from the muscles of their jaw, they must move with considerable force." These characters agree extremely well with those which we must of necessity admit in the minhocao, since it seizes very powerfully upon large animals, and drags them away to devour them. It is, therefore, probable that the minhocao is an enormous species of Lepidosiren; and we might, if this conjecture were changed into certainty, join this name to that of the minhocao, to designate the animal of the lake Feia and of the Rio des Piloes.

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* Annales des Sciences Naturelles. 2me Serie, tom. xiv. p. 116.

Zoologists who travel over these distant countries will do well to sojourn on the borders of the lake Feia, of the lake Padre Aranda, or of the Rio des Piloes, in order to ascertain the perfect truth—to learn precisely what the minhocao is; or whether, notwithstanding the testimony of so many persons, even of the most enlightened men, its existence should be, which is not very likely, rejected as fabulous.—(*Comptes Rendus*, Dec. 28, 1846.)

On the Co-existence of Ovigerous and Spermatic Capsules on the same individuals of the Hydra viridis. By ALLEN THOMSON, M.D., F.R.S.E., Professor of Physiology in the University of Edinburgh.* Communicated by the Author.

The object of the following communication is to lay before the Royal Society a few observations which I made in the months of October, November, and December of 1845, upon the occasional reproduction of the common green polype of fresh water (*hydra viridis*) by means of fecundated ova; and in particular upon the existence of spermatic and ovigerous capsules in the same individual.

I may be permitted to explain, in reference to these observations, that at the time they were made, I conceived them to be new, in so far as the bisexual condition of single individuals of the polype is concerned; but I have since found that I have been anticipated by other observers in the announcement of the fact; and I have therefore to request the Society to receive this communication rather as confirmatory and illustrative of the circumstances to which it refers, than as intended to bring forward any new discovery.

I have further to express my regret that accidental circumstances, and the only occasional occurrence of the phenomena, should have prevented me from making the observations as complete as might have been desirable.

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* Communicated and read to the Royal Society of Edinburgh, March 1847.

It appears that M. Bernard de Jussieu was the first to observe ova of the common brown polype, which he did when travelling in 1743.* M. Trembley refers to this observation, of which he was made cognizant by a private letter from M. de Réaumur. Trembley has described from his own researches these rounded egg-like bodies in his Mémoires, published in 1744; and he also mentions the occurrence on other individuals of the same species of the small conical tubercles which we now know to be the spermatic capsules. Some years later, Rösel gave an excellent coloured figure of both these bodies as they occur on separate individuals; and they were also later described by Pallas in 1766. Although the nature of the ova was suspected by Trembley, yet he expressed himself doubtingly regarding them; by some they were considered as parasites, and by others both the ova and spermatic capsules were regarded as possibly of the nature of morbid excrescences.

This state of uncertainty appears generally to have prevailed till 1836, when the nature of the ova was ascertained with greater precision by Ehrenberg, who took occasion to examine them with care in reference to a resemblance alleged by M. Turpin, to exist between the ova of Cristatella vagans and the fossil Xanthidia.

This resemblance Ehrenberg disproved, and showed that even the greater similarity that might be alleged between Xanthidia and the ova of the common polype, was only of a very general kind; considerable differences in size, form, and structure being observable between the two bodies. He further succeeded in 1838, as is stated by Mr Owen in his lectures on *Comparative Anatomy* (vol. i. p. 85), in detecting the existence of spermatozoa in the conical tubercles before referred to as existing on other individuals of the Hydra, and observed on some occasions the co-existence of the ovigerous and spermatic capsules on the same individuals. Notwithstanding these observations, the nature of the bodies in question was misrepresented by Laurent so lately as in 1842.* But very recently the fact of the occasional bisexual generation of the common polypes has been fully confirmed and described by V. Siebold† in his text-book of comparative anatomy.

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* An account of this observation was afterwards published in Abhand. der Swed. Akad, 1746, B. viii, p. 211.

I have not been able to refer to the original of the second notice regarding this subject, by Ehrenberg, which was published in the *Mittheil. aus d. Verhandl. d. Gesellschaft Naturf. Freunde in* Berlin, 1838, p. 14; and I only became acquainted with his and V. Siebold's observations a considerable time after I had observed carefully the two bodies, and distinguished their nature. The naturalists I have referred to appear to have had under observation principally the *Hydra fusca* and *H. aurantiaca;* and V. Siebold only once makes mention of having seen the ova in the Hydra viridis; and it is on this last species, the organization of which is the simplest of the tribe, that my observations have been made.

It appears to me of some importance, that the fact of the co-existence of the two generative elements in an animal of the extremely simple structure presented by the Hydra, should receive every confirmation; and the interest created by the variety of modes in which that animal is capable of being reproduced, may, perhaps, warrant my detaining the Society a few minutes with a short notice of the phenomena to which I have referred.

Through the greater part of the year the common polype is reproduced by gemmation, or by the growth of buds upon the parent stock; and occasionally, but much more rarely, by cleavage, or in the fissiparous mode; the division being sometimes longitudinal, at others transverse. These processes have been described so fully by Trembley and others, and are so familiar to most naturalists, that it seems unnecessary to dwell upon them here. Suffice it to say, that, in so far as we are yet informed from direct observation, the process of budding appears to consist in the gradual projection and growth of a portion of the entire thickness of the wall of the body in the parent polype, without its being possible to detect any peculiar structure in the bud, such as a germinal vesicle at its commencement; or without there being any evidence of fecundation by a male generative element. Whatever probability, therefore, may belong to the view held by some, that buds require fecundation as ova do, this opinion cannot be considered as founded on observation in the polype; an animal, it may be remarked, in which a most favourable opportunity presents itself of detecting such a fecundation were it to occur.

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* Frorieps Neue Notizen, p. 104.

† *Lehrbuch der Vergleich. Anatomie,* Von V. Siebold und Stannius, B. i. p. 46.

Buds are formed upon those polypes in which the spermatic capsules exist; but they are much more frequently observed in those individuals which do not possess them, and I regard the assertion of Professor Owen, that the process of gemmation is preceded by the fecundation of an ovum below the integument, as not yet warranted by observation. I have never, at least, been able to detect any appearances which would lead me to believe that such was the case. In regard to this matter, however, it may be proper to notice that the place from which, in the gray polype, the buds almost always sprout, is the same with that in which the ova are developed—viz., at the union of the pedicle and body of the animal; the spermatic capsules being in general nearer the head, and removed from the ova or buds.

From Trembley's interesting researches, it appears necessary, in order that germination should go on briskly, that the polypes should be well nourished; and when this is the case, and the weather is fine, a very considerable number of buds, and even a second generation, may appear on some of the progeny of the first parent: and that, on the other hand, when food is withheld for some time, there is an increased tendency to the separation of the buds, so that some occasionally leave the parent stalk, while yet very imperfectly developed.

It is in the autumn principally that the formation of ova and spermatic capsules occurs; and it is at this season or in winter that almost all observers have described the process. Ehrenberg is, however, an exception, mentioning that he has seen the ova formed in the month of June. A succession of ova are observed to be formed on, and detached from, the same polype; and according to V. Siebold, the parent generally dies after the separation of the last of the series. The development of fecundated ova, therefore, at the approach of winter, may be regarded as a means of security against the extinction of the species through the death of the entire animals by cold or want. The ova probably remain for a considerable time in an undeveloped condition.

The extreme simplicity of the organs in question is a feature of some interest. In order to explain this, I may state that the wall of the body of the hydra, though endowed with a digestive and assimilative faculty, and possessed of sensitive and motor powers to a remarkable degree, is yet composed of no other structure than organised cells united together by their adjacent parietes, so as to form two layers; of which the innermost contains the green granules, which give colour to the most of the animal; and the outer presents, studded over its surface, those peculiar forms of cells from which the stinging or poison filaments protrude, with which the animal, in seizing its prey with its tentacula, appears at once to stupify and to fasten them. No nervous or muscular fibre, no special glandular organs, are visible; but every cell for itself, and the whole mass of them in concert, appear to be capable of the acts of assimilation, and to execute their movements under the influence of sensations by a texture of a similar cellular organization throughout.

The development, therefore, from so simple a texture of organs possessing so obvious and peculiar a form and structure as the spermatic and ovigerous capsules, is a very striking phenomenon.

The conical eminences which constitute the spermatic capsules appear to consist, in the greater degree of development, of one or more of the superficial cells in the vicinity of the base of the arms. These capsules sometimes occur in considerable numbers, as from eight to sixteen on the brown polype;

but in the green species I have in general seen only two or three, placed alternately on opposite sides of the body. The interior of the capsule has a slightly ribbed or striated appearance, and at the summit a small aperture is sometimes perceptible, through which, when the development is complete, the spermatic filaments are observed to issue. In breaking up the capsule, under the microscope large numbers of these filaments are seen united in bundles by their minute globular heads, the filamentous part being free, and vibrating with great rapidity, in the manner which is known to be characteristic of these bodies in all animals.

I have noticed more than once, when an ovum was developed on the same individual which bore the spermatic capsules, the animal bend its body suddenly round, so as quite to double it, and to bring the spermatic capsules and ovum close together; and as this is an attitude rarely observed in the polype at other times, I am inclined to regard it as related to the act of self-fecundation.

The spermatic capsules, with lively movement of their filaments, were observed on many individuals in which no ova existed.

The rounded body, which I have hitherto termed, indiscriminately, ovum and ovigerous capsule, is, when fully developed, of such a size as to be seen with the naked eye. It is attached to the side of the polype, nearer the foot than the spermatic capsules, and is distinguished from the rest of the animal by its spherical form and yellowish-brown colour. In the Hydra viridis I have never seen more than one of these ova developed on the body of the polype at the same time; but a number varying from four to seven have been observed by others upon the Hydra fusca.

This body appears at first as a small granular mass in the thickness of the wall of the animal. A germinal vesicle was not apparent; but as I found it impossible to burst the capsule to examine the contents without injuring them greatly, it may still have existed within the granular mass. As the spherical yolk mass enlarged, it projected from the side, seeming at first to carry along with it the outer or clearer layer of the animal's body; then the cells of this layer seemed to become thinner, and to recede from the outer covering or capsule enveloping the egg-like mass, which had at the same time become much thicker, and was now left attached to the animal by a narrower portion or pedicle. In the further progress of development a similar solution or atrophy of the cells of the pedicle was followed at last by the separation of the spherical mass, which fell from the body of the polype to the bottom of the vessel in which it was contained.

The outer covering of this mass or capsule is described by Ehrenberg as presenting in the *Hydra aurantiaca*, on its outer surface, a remarkable set of blunt prickles, many of which are bifid at the end. In the *Hydra viridis* it forms a very strong, elastic, and almost horny-like capsule, the surface of which is rather tuberculated than spinous. The divisions between the tubercles are irregularly hexagonal or pentagonal, and their walls appear to contain a fibre, wound spirally round them.

In different capsules, while still attached to the parent animal, I observed the internal yolk or granular mass to become subdivided into a number of smaller portions, each of which was of a rounded form, and had a darker centre or nucleus. From similar observations by V. Siebold, I am inclined to regard the fluid granular mass within the capsule as the yolk of a single ovum, and the smaller masses into which I have mentioned it became divided, as the result of that peculiar process of cleavage of the substance of the yolk, and the arrangement of the distinct portions round separate centres, which was first discovered by Prevost and Dumas in Batrachia, and by Barry in Mammalia, and which is now known to occur under certain modifications in the ova of all animals previous to the formation of an embryonic membrane. I am, therefore, disposed to consider the whole mass which is developed from the side of the polype in the manner before stated, as constituting a single ovum, and as thus different from the multiple ovigerous capsules of the Coryne, and some other allied polypes.

I preserved several of the separated ova for some time, but was not so fortunate as to succeed in tracing the development of a young polype from any of them; having accidentally lost two, in which I hoped to have had an opportunity of watching the process. Trembley* mentions his having observed, as he conceived, the formation of a young polype from the egg; and Pallas † and Laurent ‡ make a similar statement; but none of these naturalists express themselves with sufficient confidence, nor describe the process with sufficient detail, to satisfy us fully of the fact.

Dr Thomas Wright, of this place, who has recently devoted some time to the study of the structure and habits of these interesting animals, informs me that he examined the ovigerous capsules of the long-armed polype (*Hydra aurantiaca*) last autumn, and watched their gradual formation on the body of the animal. He observed two of these capsules, of a cream colour, developed on one individual, one of which became detached. From this, when it was burst artificially some days afterwards, there issued a number of ovoidal ciliated bodies, which moved with vivacity through the water, and which, he at first imagined, might be separate ciliated ova, like those of some other polypes. This appears to me, however, extremely doubtful; and Dr Wright states, in a communication he has made to me on the subject, that the polype appeared to be in an unhealthy state; and I am inclined to think that the ciliated bodies were polygastric animalcules such as I have repeatedly seen preying with avidity upon the bodies of polypes when in a very weak condition, or in commencing decomposition.

Lastly, it may be stated that while some of the individuals of the species *Hydra* are, as I have now described them, bisexual or hermaphrodite, others appear to bear the organs of one sex only.

The observations I have detailed suggest some general reflections of interest. With many others of the same kind, they point out the multitude of the resources of Nature, and almost lavish expenditure of her care in providing secure means for the continuance of the species of animals. They call our attention to the remarkable fact of the existence of two distinct kinds of generative elements, even among the simplest of animals. They thus add probability to the view deducible from an extended consideration of the recent observations in the vegetable as well as the animal kingdom, that in no instance is a new organized structure, under the form of an ovum, seed, or spore, separated from a parent, and made capable of producing a new being, without the concurrence of generative elements of two kinds—one of these being itself a cell, or in a vesicular form; the other being a peculiar product of cell development, and most frequently assuming the form of minute filaments endowed with a power of rapid vibratile motion. Lastly, in contemplating the alternate production of buds and ova from the same situation in these polypes, they suggest the interesting speculative inquiry whether the concurrence of a male element is necessary to give fecundity to the germ of a mere bud—a view in regard to which, although some circumstances appear to give a show of probability, the want of sufficient observations forces us in the meantime to suspend our judgment.

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* Memoires. † Pallas, Karakteristik der Thierpflanzen, p. 53.

‡ Laurent, in Frorieps Neue Notizen, No. 513, p. 101.

Description of the Plate.

Figure I. Hermaphrodite individual of the Hydra viridis attached by its foot to a portion of the root of a Lemna; *a*, ovigerous capsule; *bb*, spermatic capsules, magnified ten diameters.

Figure II. The ovigerous capsule burst, its granular contents escaping.

Figure III. The spermatic capsule burst, with some of the spermatic filaments in fasciculi near it. (The two last figures are more highly magnified than the first.)

On the Glacial Theory, and the Effects of Glacial Action.

Communicated by the Author.

The history of the Glacial Theory affords another example of that apparent fatality which has ever attended the advancement of each new discovery in science. The evidence of the former extension of glaciers has now been sufficiently long before the public to enable them to form a correct opinion upon the merits of the theory, either to acknowledge its truth or to expose its fallacy. The error which its opponents appear to labour under lies in not dealing with the established properties of matter in the same manner as they would have done, had another and more familiar subject been under discussion, but in assuming that matter possesses exactly those properties which are suitable for their preconceived views. Such was the case with the reasons first assigned for the motion of glaciers. Had De Saussure applied the laws which regulate the descent of bodies down inclined planes rigorously to the motion and condition of glaciers, he would have found that the sliding theory was inapplicable; and had M. Agassiz considered the temperature to which water must be reduced before it can freeze, he would not have advocated the dilatation theory; nor would he have asserted with so much confidence that the motion is suspended during winter, had that not been essential to his theory. Professor Forbes is the only one who has investigated the causes of the motion of glaciers in a manner likely to lead to the truth, and his exertions have accordingly been crowned with success. We, however, paused until lately to subscribe to the viscous theory, notwithstanding the many *prima facie* proofs he had given in support of it, because we considered that the *experimentum crucis* was still wanting—we mean the proof of the power which glaciers were thought to possess of ejecting upon their surface foreign bodies from their interior. A thousand obvious facts prove, that a fluid in motion raises from its bottom bodies having a greater specific gravity than itself; and if the same property could be shown to exist in glaciers, we deemed that the viscous theory was proved. Happily, Professor Forbes has been fortunate in supplying this remaining link of the chain, by the discovery last summer of a knapsack on the surface of the Mer de Glace of Chamouni, which had been left in a crevass, the account of which he gave in the last number of this Journal. We trust that the viscous theory is now settled.

We wish we could say as much for the glacial theory. Believing in the former extension of glaciers, and that it was from their agency alone that the most important changes in the earth's surface have been effected, and that the opposition made to it proceeds from a misapprehension of the power of water, we shall endeavour to point out two assumptions which appear to have been taken as facts, or as axioms whereon to build a demonstration, instead of considering them merely as propositions to be proved.

The opponents of the glacial theory assume, as the basis of their arguments,

1st, That water has an abrading action upon rocks.

2dly, That stones, propelled by water, can make furrows and scratches upon rocks with which they may come in contact, similar to those which are made by stones propelled by glaciers.

If water has the abrading power which some attribute to it, we should not fail to find at the mouths of rivers which have run for a considerable distance over a gravelly bed, some indications in the water of the abrasion; and we should find the most perfect specimens of rounded stones at the mouths of rivers; and, as we ascended the stream, we should find them becoming less and less rounded, the most angular and irregular in shape being at the source. Now, neither of these is the case. The water at the mouths of rivers with gravelly beds (except in times of floods) is as free from particles of stone as when it issues from its spring, and the gravel there is as round and smooth as at its mouth. The effects produced upon rocks which intercept the passage of a river, we have not overlooked. Some are worn, and the stream rushes through the cavity or fissure formed; others are covered with a vegetable substance, and the rock beneath is as rough as on the day it was first bared. In the former case, the stream has descended over a gravelly bed, and we conclude that the fissure has been formed from the attrition of the stones which the water may have brought down with it in its floods; and, in the latter case, it has descended over a sandy bottom, from which we conclude, that so little abrasive power has the water, even when charged with particles of sand, that it does not prevent the seeds of plants germinating upon the rock.

But, whatever may be the abrasive power of water, it has never been shown that a stone propelled by water can make furrows and scratches upon the rocks with which it may come in contact. We believe this is physically impossible; and that, if it did exist, there are no rivers which would not afford numerous examples of such furrows and scratches. Rocks might be polished, gravel might be formed, if water had the power of abrasion; or these might be produced by the friction of stones upon each other, if continued long enough; but a furrow or a scratch can only be made under a pressure sufficient to overcome the cohesive force of the particles of the body acted upon. When we have the moving power at will, we can, by increasing it, cause a less dense body to mark a denser one. We can force a candle through a deal of wood, or a leaden bullet through a plate of iron; but where the power is a stream of water, whose velocity practically lies within a very limited range, and when the body moved is a rock or stone, which has lost by its emersion from one-half to one-third of its weight, the force with which it impinges against any obstacle in a stream, however impetuous, must be comparatively very slight, and wholly insufficient to produce a furrow "six feet long, an inch wide, and one-third of an inch deep," as found on Arthur's Seat.

Nor will time render any assistance in this operation. The grooves and scratches clearly indicate that their formation was completed at one stroke of the tool, and not by a succession of strokes with different tools. Indeed, the probability of two or more stones moving along the side of a rock exactly in the same place, is so very remote, that it cannot for an instant be seriously entertained.

We need not discuss this question with reference to the grooves and scratches which are found upon the sides of rocks several hundred feet above the bed of the valley beneath, and thousands above the level of the sea. For if it be clearly shown that water has the power of producing them in the lower situations where they are found, that is all that can fairly be required from the opponents of the theory; without asking them to show from whence such a body of water came which produced the higher scratches. But we think it incumbent upon them to tell us what was the velocity of the stream which could raise and keep, for perhaps a horizontal distance of six feet, a block of porphyry or basalt, even a few inches above the bed of the river.

There is another point which the opponents of the glacial theory would do well to consider—the agent which has produced all the earthy materials of soil. If the mouldering of rocks by the influence of the atmosphere, and the supposed abrasive power of water, have been the principal agents engaged, millions of years would not have sufficed to produce the soil which covers the smallest of the English counties. The Falls of Niagara afford an admirable illustration of the effects of water; and perhaps no example could be found more favourable to the views of the advocates for the abrasive power of water, if such a power exists. It is conjectured, and apparently with much truth, that the Falls were once at Queenston, about seven miles down the river, and that they are slowly receding up the river.* Mr Lyell thinks the rate of recession may be estimated at a foot annually, and, consequently, that it is 35,000 years since the Falls were at Queenston. According to the same author, the ravine in which the river flows, is at the Falls cut perpendicularly through 80 feet of limestone rock, and through the same thickness of shale; and at Queenston it is cut through 100 additional feet of sandstone. Now, the depth of the Falls is about 165 feet; and assuming that the average breadth of the ravine between the Falls and Queenston is 3000 feet, (which is more than the width of the two falls,) and that Queenston is 100 feet below the level of the bottom of the Falls, 2649 millions of cubic yards of rock have been excavated to form the ravine; or, in 35,000 years only as much material has been removed as would cover a space 30½ miles long and four broad, seven yards thick. But the whole of the excavated material would not be sand or abraded rock; much of it would be detached in considerable masses; and when that portion is deducted, the quantity of sand produced even by that large river, and in so long a time, must be quite insignificant. The Niagara river ought also to afford upon the sides of the ravine, numerous scratches and furrows; and, if there were any, they could scarcely have escaped the searching eye of Lyell; but we venture to predict that not the slightest trace of a scratch will there be found.

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* Travels in America.

Some there are, who, while they admit that the striæ of the rocks upon the continent are the effects of glacial action, deny that the similar appearances in this country are due to the same agent, because they cannot perceive the mountains here from which the glaciers have emanated. This seems to us the weakest of all arguments, for it presupposes that the British Isles are in the same condition now, in regard to their hills and valleys, that they were in the remoter ages—a supposition which is contradicted by a multitude of geological facts. But the same objection would apply to the striæ being formed by a violent rush of water, which it is thought produced them. For, can there be a torrent of water without a declivity? and where are the mountains, from which such a volume of water could issue as would produce such effects?

We are not aware that any experiments have yet been instituted to ascertain the quantity of sand and land brought from glaciers by the water which issues from them; but, judging from its turbid appearance, the quantity must be very considerable. There is this distinction between the sand in glacier water and that in river water, that, in the former, each particle of sand is a new accession to the soil of the earth; whereas in the latter, it is merely a removal of the soil of one part of the earth to another. We have therefore undeniable evidence that the present glaciers are adding soil to the earth; and, consequently, we have an existing agent capable of producing by the abrasion of rocks, all the earthy material of soil. In rivers and currents we have the agent by which that soil is distributed on the face of the earth. Until we saw what glaciers were performing, and the evidence which exists of their former great extension, the agent by which rocks were ground into soil, was unknown to us. But when we saw sharp-edged stones falling into crevasses, and appearing at the extremity of the glacier rounded, polished, and striated—when we saw the beautifully clear water of the glacial rivulets descending into the same crevasses, and gushing from the same extremity a turbid stream, the explanation was before our eyes—the conclusion was irresistible—these were the MANUFACTORIES OF SOIL.

ALNWICK,
3d March 1847.

*Facts relating to the Great North American Lakes.** By Professor C. DEWEY.

1. *Phenomenon on Lake Ontario.*

On 20th September 1845, was witnessed a singular phenomenon on Lake Ontario. In the afternoon the waters suddenly moved, in a mass, out of the rivers, bays, coves, harbours, &c., lowering the water to different depths in different places. In ten or twelve minutes the waters returned, and rose to a higher level than they had before. This oscillation, or efflux and reflux of the waters, was repeated several times, at about the same interval of eight or twelve minutes. At the mouth of the Genesee river, seven miles from the city, the water fell two feet below its common level, and soon rose as much above it. The Revenue cutter, John Y. Mason, lay in the harbour, and the hands witnessed the fall and rise of the waters. At several places along the neighbouring shores, boats were left for a few moments on the sands. At Oswego, seventy miles east of this, a large body of logs moved out into the lake, to the great annoyance of the owner, till he saw them soon returning to their previous location. At Cobourg, *a little west of the Genesee*, and on the Canada side of the lake, and *distant about sixty miles*, the same fall and rise were observed to be repeated, the greatest being a little before sunset, when the waters rose to their highest point, or about two feet. In the efflux, the shores had in many places been left dry for some minutes. At Port Hope, a few miles west of Cobourg, the steam-boat Princess Royal ran aground as she attempted to enter the harbour, so much had the waters lowered in the port.

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* Originally printed in the Report of the Regents of the University New York, in 1846, p. 228, nearly as prepared by the author for Silliman's Journal.

The cause of this phenomenon is doubtless to be found in the tornado which passed that afternoon over the lake, beginning at Johnston's Creek in Niagara County, and passing in a north-east course over Orleans County, till it struck the lake at Oak Orchard Creek, fifty miles west of Rochester, and continued its course across the lake. The tornado was about three-fourths of a mile wide in Orleans County, and was very destructive, twisting off and bearing away large trees, unroofing and destroying buildings, &c. Its violence was of only a few minutes' duration, perhaps not over three. On the lake it produced water-spouts, and was attended with large hail, and lightning and thunder. The steam-boat Express, Captain Mason, was in great jeopardy from the wind and waves, and storm, as she was then passing up the lake on her regular trip. The power of this tornado was probably sufficient to withdraw the waters from the shores, so as to produce the efflux and reflux that was witnessed. Such sudden changes of the level of the waters are said to have been witnessed before on the lakes. The solution in this case may apply to the whole. It seemed desirable to collect and connect the facts in this case, and to publish them, as they prevent a resort to supposed earthquakes heaving the bottom of the lakes, or change of the level of the shores, of which not a trace is left, and not a probability exists. This tornado does not appear to have moved rapidly, but to have derived its force from the great rotatory velocity, as it twisted off trees, breaking them more than overturning them. A lumber waggon was raised into the air, and carried a considerable distance. A stick of timber, which required eight men to carry it was removed to the distance of fifty roods. On the lake, e water-spouts were raised, and a great body of water ned to be elevated into the air. It passed over nearly the dle of this part of the lake, as it was too far from Cobourg the north side, and from Rochester on the south side, to act any attention.

2. *Fall of the Water of the Lakes.*

t is well known that the level of Lake Ontario slowly cended through several months of last year. The collector the port of Genesee, L. B. Langworthy, caused accurate asurements to be made at the mouth of the river, which e been continued by his successor. The level was consi- ably lower last summer than in several years before; and level fell till the beginning of this year. About the 1st of rch it began to rise, as shown by the following record from collector's office: —

			Feet.	Inches.
1845.—June 1,	Water below	top of Wharf	1	0
Sept. 1,	ditto	ditto	2	1
Oct. 1,	ditto	ditto	2	6
Nov. 1,	ditto	ditto	2	9
Dec. 1,	ditto	ditto	3	1
1846.—Jan. 1,	ditto	ditto	3	4
Feb. 1,	ditto	ditto	3	4
Mar. 1,	ditto	ditto	3	2
April 1,	ditto	ditto	2	9
May 1,	ditto	ditto	2	6
— 21,	ditto	ditto	2	3

e melted snows of March, and rains since, have made a rise more than a foot.

The level of the water has been considerably lower than al in Lake Erie, and at Detroit, and in Lake Michigan, at Niagara Falls. The same fact is doubtless true in ke Superior; for the same reason has been operating over the region of the great lakes. An uncommon drought for more than half of 1845 extended over this whole section. Of course, far less *water than is common was poured into the lakes.* The cause of the fall is most obvious; and there can be no necessity for a resort to the notion of a regular and periodical rise and fall of these inland seas, or to a supposition of an actual sinking of the bottom of their outlets. While the drought extended over a wide extent of our country, the seasons were wet, and rain was abundant in the western parts of Europe.—(*American Journal of Science and Arts*, 2d series, No. 4, July 1846.)

On Guano (*Huanu*).

Opposite to Pisco and Chinca there is a group of small islands, of which the largest, Tangallan, is six English miles distant from Pisco. These islands have, of late years, become celebrated on account of the great quantity of guano that has been exported from them.

Guano (or, according to the more correct orthography, Huanu*) is found on these islands in enormous layers of from thirty-five to forty feet thick. The upper strata are of a greyish-brown colour, which, lower down, becomes darker. In the lower strata the colour is a rusty red, as if tinged by oxide of iron. The Guano becomes progressively more and more solid from the surface downward, a circumstance naturally accounted for by the gradual deposit of the strata, and the evaporation of the fluid particles. Guano is found on all the islands, and on most of the uninhabited promontories of the West of South America, especially in those parts within the tropics. I have often been assured that beds of Guano, several feet high, covered with earth, are found inland at some distance from the sea; but I never met with any, and I have some doubts of the correctness of the statement. If, however, these inland strata really exist, I am inclined to believe that they can only be found on hilly ground; and in that case they afford strong evidence of a considerable elevation of the coast.

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* The original word is Huanu, which is a term in the Quichua dialect, meaning "animal dung;" for example, *Huanacuhuanu* (excrement of the Huanacu). As the word is now generally used, it is an abbreviation of *Pishu Huanu,* bird-dung. The Spaniards have converted the final syllable *nu* into *no,* as they do in all the words adopted from the Quichua which have the like termination. The European orthography, *Guano,* which is also followed in Spanish America, is quite erroneous, for the Quichua language is deficient in the letter *G,* as it is in several other consonants. The *H* in the commencement of the word is strongly aspirated, whence the error in the orthography of the Spaniards, who have sadly corrupted the language of the Antochthones *of Peru.*

Guano is formed of the excrements of different kinds of marine birds, as mews, divers, sheerbeaks, &c.; but the species which I can name with more precision are the following:—*Larus modestus*, Tsch.; *Rhinchops nigra*, Lin.; *Plotus Anhinga*, Lin.; *Pelecanus thayus*, Mol.; *Phalacrocorax Gaimardii*, and *albigula*, Tsch. (*Pelecanus Gaimardii*, Less; *Carbo albigula*, Brandt); and chiefly the *Sula variegata*, Tsch.

The immense flocks of these birds, as they fly along the coast, appear like clouds. When their vast numbers, their extraordinary voracity, and the facility with which they procure their food, are considered, we cannot be surprised at the magnitude of the beds of Guano, which have resulted from uninterrupted accumulations during many thousand years. I kept for some days a living *Sula variegata*, which I fed abundantly with fish. The average weight of the excrement daily was from three-and-a-half to five ounces. I have no doubt that when the bird is in a state of freedom, the weight must be much greater; for these birds are constantly plunging into the sea, in order to devour the fishes, which they find in extraordinary masses around all the islands. When an island is inhabited by millions of sea-birds, though two-thirds of the Guano should be lost while flying, still a very considerable stratum would be accumulated in the course of a year.

The marine birds nestle on the uninhabited islands, or on rocks near the shore; but they never settle on the flat beach, or any place distant from it inland. On this fact I ground my conjecture, that those beds of Guano in the interior, which may have been removed from the shore by important elevations of the coast, are to be found only on hills.

During the first year of the deposit the strata are white, and the guano is then called *Guano blanco.* In the opinion of the Peruvian cultivators, this is the most efficacious kind. It is found in the Punta de Hormillos, on the islands of Islay, Jesus, Margarita, &c.

As soon as the dealers in guano begin to work one of the beds, the island on which it is formed is abandoned by the birds. It has also been remarked, that since the increase of trade and navigation, they have withdrawn from the islands in the neighbourhood of the ports.

Much has recently been written on the employment and utility of guano; but the manner in which it is employed as manure in Peru, seems to be but little known. The Peruvians use it chiefly in the cultivation of maize and potatoes; a few weeks after the seeds begin to shoot, a little hollow is dug round each root, and is filled up with guano, which is afterwards covered with a layer of earth. After the lapse of twelve or fifteen hours, the whole field is laid under water, and is left in that state for some hours. Of the *Guano blanco* a less quantity suffices, and the field must be more speedily and abundantly watered, otherwise the roots would be destroyed. The effect of this manure is incredibly rapid. In a few days the growth of the plant is doubled; if the manure be repeated a second time, but in smaller quantity, a rich harvest is certain;—at least, the produce will be threefold that which would have been obtained from the unmanured soil.

The haciendas of the valley of Chancay have, during the last fifty years, consumed annually 33,000 to 36,000 bushels of guano brought from the islands of Chancha and Pisco.

The price of the bushel of coloured guano is one dollar and a quarter, and the price of the white from two to three dollars. The price has recently undergone many fluctuations, in consequence of the great exports to Europe.

The employment of this kind of manure is very ancient in Peru; and there is authentic evidence of its having been used in the time of the Incas. The white guano was then chiefly found on the islands opposite to Chincha; so that for upwards of six hundred years the deposit has been progressively removed from those islands without any apparent decrease of the accumulation. The uniformity of climate on a coast where there is not much rain, must contribute to render the Peruvian guano a more arid manure than the African, as fewer of the saline particles of the former being in solution, they are consequently less subject to evaporation.—(*Dr Tschudi's Travels in Peru*, p. 239.)

On Coca (Erythroxylon Coca, Lam.) By Dr J. J. VON TSCHUDI.

The coca (*Erythroxylon coca*, Lam.) is a shrub about six feet in height, with bright green leaves and white blossoms; the latter are succeeded by small scarlet berries. It is raised from the seed, in garden-beds called almazigas. When the young shoots are one and a half or two feet high, they are removed to regularly laid out coca fields (*cocales*), where they are planted at the distance of about three spans from each other. The coca requires humidity; therefore, during the first year or two after it is planted in the fields, maize is sown between the *matas*, or young shoots, to screen them from the too great influence of the sun. When the leaves are ripe, that is to say, when, on being bent, they crack or break off, the gathering commences. The leaves are stripped from the branches, a task usually performed by women; and it requires great care lest the tender leaves and young twigs should be injured. In some districts the Indians are so very careful in gathering the coca, that, instead of stripping off the leaves, they cut them from the stem by making an incision with their nails; the plant, thus rendered leafless, is soon again overgrown with verdant foliage. After being gathered, the leaves are spread out on coarse woollen cloths, and dried in the sun. The colour of the leaves when dried is a pale green. The drying is an operation which likewise demands great care and attention; for if the leaves imbibe damp, they become dark coloured, and then they sell for a much lower price than when they are green. The dry coca is finely packed in woollen sacks and covered with sand. These sacks are of various sizes and colours, in different parts of the Montanas. In Huanuco they are grey or black, and, when filled, weigh from seventy-five to eighty pounds. In Vitoc, they are grey and white, and contain one hundred and fifty pounds. In Huanto and Anco, they are small in size, and black or brown in colour, and contain merely one aroba. In the Montanas of Urubamba, Calca, and Paucartambo, the coca leaves are put in small baskets, called *cestos,* and covered with sand. Great care is also requisite in the carriage of the coca, for if damp be allowed to penetrate the sack, the leaves become hot, or, as the natives express it, *Se calientan,* and are thereby rendered useless.

The Indians masticate the coca. Each individual carries a leathern pouch called the *huallqui*, or the *chuspa*, and a small flask gourd, called the *ischupuru.* The pouch contains a supply of coca leaves, and the gourd is filled with pulverised unslaked lime. Usually four times, but never less than three times a-day, the Indian suspends his labour, for the purpose of masticating coca. This operation (which is termed *chacchar* or *acullicar*) is performed in the following manner:—Some of the coca leaves, the stalks having been carefully picked off, are masticated until they form a small ball, or, as it is called, an *acullico.* A thin slip of damp wood is then thrust into the *ishcupuru*, or gourd, and when drawn out, some portion of the powdered lime adheres to it. The *acullico*, or ball of masticated coca leaves, is, whilst still lying in the mouth, punctured with this slip of wood, until the lime mixing with it, gives it a proper relish, and the abundant flow of saliva thus excited, is partly expectorated and partly swallowed. When the ball ceases to emit juice, it is thrown away, and a new one is formed by the mastication of a fresh mouthful of coca leaves. In Cerro de Pasco, and in places still farther south, the Indians use, instead of unslaked lime, a preparation of the pungent ashes of the quinua (*Chenopodium Quinua*, L.) This preparation is called *Llucta* or *Llipta.* In using it, a piece is broken off, and masticated along with the *acullico.* In some of the Montana regions the Llucta is made from the ashes of the musa root. The application of the unslaked lime demands some precaution, for if it comes in direct contact with the lips and gums, it causes a very painful burning. During a fatiguing ride across the level heights, where, owing to the cold wind, I experienced a difficulty of respiration, my arriero recommended me to chew coca, assuring me that I would experience great relief from so doing. He lent me his *huallqui*, but, owing to my awkward manner of using it, I cauterised my lips so severely that I did not venture on a second experiment.

The flavour of coca is not unpleasant. It is slightly bitter, aromatic, and similar to the worst kind of green tea. When mixed with the ashes of the musa root, it is somewhat pungent, and more pleasant to European palates than it is without that addition. The smell of the fresh-dried leaves in a mass is almost overpowering; but this smell entirely goes when they are packed in the sacks. All who masticate coca, have a very bad breath, pale lips and gums, greenish and stumpy teeth, and an ugly black mark at the corners of the mouth. An inveterate *coquero*, or coca chewer, is known at the first glance. His unsteady gait, his yellow-coloured skin, his dim and sunken eyes encircled by a purple ring, his quivering lips, and his general apathy, all bear evidence of the baneful effects of the coca juice when taken in excess. All the mountain Indians are addicted more or less to the practice of masticating coca. Each man consumes, on the average, between one ounce and an ounce and a half per day, and on festival days about double that quantity. The owners of mines and plantations allow their labourers to suspend their work three times a-day for the *chacchar*, which usually occupies upwards of a quarter of an hour; and after that they smoke a paper cigar, which crowns the zest of the coca mastication. He who indulges for a time in the use of coca, finds it difficult, indeed almost impossible, to relinquish it. This fact I saw exemplified in the cases of several persons of high respectability in Lima, who are in the habit of retiring daily to a private apartment for the purpose of masticating coca. They could not do this openly, because among the refined class of Peruvians the chacchar is looked upon as a low and vulgar practice, befitting only the labouring Indians. Yet Europeans occasionally allow themselves to fall into this habit; and I knew two in Lima, the one an Italian, and the other a Biscayan, who were confirmed coqueros in the strictest sense of the word. In Cerro de Pasco there are societies, having even Englishmen for their members, which meet on certain evenings for the chacchar. In these places, instead of lime or ashes, sugar is served along with the coca leaves. A member of one of these clubs informed me that on the few first trials the sugar was found very agreeable, but that afterwards the palate required some more pungent ingredient.

The operation of the coca is similar to that of narcotics administered in small doses. Its effects may be compared to those produced by the thornapple, rather than to those arising from opium. We shall afterwards notice the consequences resulting from drinking the decoction of the datura. In the inveterate coquero similar symptoms are observable, but in a mitigated degree. I may mention one circumstance attending the use of coca, which appears hitherto to have escaped notice —it is, that, after the mastication of a great quantity of coca, the eye seems unable to bear light, and there is a marked distension of the pupil. I have also observed this peculiarity of the eye in one who had drunk a strong extract of the infusion of coca leaves. In the effects consequent on the use of opium and coca there is this distinction, that coca, when taken even in the utmost excess, never causes a total alienation of the mental powers, or induces sleep; but, like opium, it excites the sensibility of the brain, and the repeated excitement occasioned by its intemperate use after a series of years, wears out mental vigour and activity.

It is a well-known fact, confirmed by long observation and experience, that the Indians who regularly masticate coca, require but little food, and, nevertheless, go through excessive labour with apparent ease. They therefore ascribe the most extraordinary qualities to the coca, and even believe that it might be made entirely a substitute for food. Setting aside all extravagant and visionary notions on the subject, I am clearly of opinion that the moderate use of coca is not merely innoxious, but that it may be even very conducive to health. In support of this conclusion, I may refer to the numerous examples of longevity among the Indians who, almost from the age of boyhood, have been in the habit of masticating coca three times a-day, and who, in the course of their lives, have consumed no less than two thousand seven hundred pounds, yet, nevertheless, enjoy perfect health.* The food of the Indians consists almost exclusively of vegetable substances, especially roasted maize, and barley converted into flour by crushing, which they eat without the admixture of any other substance. The continued use of this farinaceous food occasions severe obstructions, which the well-known aperient qualities of the coca counteract, and many serious diseases are thereby prevented. That the coca is in the highest degree nutritious, is a fact beyond dispute. The incredible fatigues endured by the Peruvian infantry, with very spare diet, but with the regular use of coca; the laborious toil of the Indian miner, kept up under similar circumstances, throughout a long series of years, certainly afford sufficient ground for attributing to the coca leaves not a quality of mere temporary stimulus, but a powerful nutritive principle. Of the great power of the Indians in enduring fatigue, with no other sustenance than coca, I may here mention an example. A cholo of Huari, named Hatun Huamang, was employed by me in very laborious digging. During the whole time he was in my service, viz., five days and nights, he never tasted any food, and took only two hours' sleep nightly. But at intervals of two and a half or three hours, he regularly masticated about half an ounce of coca leaves, and he kept an acullico continually in his mouth. I was constantly beside him, and therefore I had the opportunity of closely observing him. The work for which I engaged him being finished, he accompanied me on a two days' journey of twenty-three leagues, across the level heights. Though on foot, he kept up with the pace of my mule, and halted only for the *chacchar*. On leaving me, he declared that he would willingly engage himself again for the same amount of work, and that he would go through it without food, if I would but allow him a sufficient supply of coca. The village priest assured me that this man was sixty-two years of age, and that he had never known him to be ill in his life.

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* I allude here to individuals (and such cases are by no means singular) who have attained the great age of one hundred and thirty. Supposing those Indians to have begun to masticate coca at ten years old, and calculate their daily consumption as a minimum at one ounce, the result is the consumption of twenty-seven hundredweight in one hundred and twenty years.

The Indians maintain that coca is the best preventive of that difficulty of respiration felt in the rapid ascents of the Cordillera and the Puna. Of this fact I was fully convinced by my own personal experience. I speak here, not of the mastication of the leaves, but of their decoction, taken as a beverage. When I was in the Puna, at the height of 14,000 feet above the level of the sea, I drank always, before going out to hunt, a strong infusion of coca leaves. I could then, during the whole day, climb the heights and follow the swift-footed wild animals without experiencing any greater difficulty of breathing than I would have felt in similar rapid movements on the coast. Moreover, I did not suffer from the symptoms of cerebral excitement or uneasiness which other travellers have observed. The reason perhaps is, that I only drank this decoction in the cold Puna, where the nervous system is far less susceptible than in the climate of the forests. However, I always felt a sense of great satiety after taking the coca infusion, and I did not feel a desire for my next meal until after the time at which I usually took it.

By the Peruvian Indians the coca plant is regarded as something sacred and mysterious, and it sustained an important part in the religion of the Incas. In all ceremonies, whether religious or warlike, it was introduced, for producing smoke at the great offerings, or as the sacrifice itself. During divine worship the priests chewed coca leaves; and unless they were supplied with them, it was believed that the favour of the gods could not be propitiated. It was also deemed necessary that the supplicator for divine grace should approach the priests with an *acullico* in his mouth. It was believed, that any business undertaken without the benediction of coca leaves, could not prosper; and to the shrub itself worship was rendered. During an interval of more than three hundred years, Christianity has not been able to subdue the deep-rooted idolatry; for everywhere we find traces of belief in the mysterious powers of this plant. The excavators in the mines of Cerro de Pasco throw masticated coca on hard veins of metal, in the belief that it softens the ore, and renders it more easy to work. The origin of this custom is easily explained, when it is recollected that, in the time of the Incas, it was believed that the *Coyas*, or the deities of metals, rendered the mountains impenetrable, if they were not propitiated by the odour of coca. The Indians, even at the present time, put coca leaves into the mouths of dead persons, to secure to them a favourable reception on their entrance into another world; and when a Peruvian Indian on a journey falls in with a mummy, he, with timid reverence, presents to it some coca leaves as his pious offering.

Soon after the conquest of Peru, when the Spaniards treated the Indians and all their customs with contempt, coca became an object of aversion to the Whites. The reverence rendered by the natives to the coca plant, induced the Spaniards to believe that it possessed some demoniacal influence. The officers of the government and the clergy, therefore, endeavoured, by all possible means, to extirpate its use; and this is one cause, hitherto overlooked, of the hatred with which the Indians regarded the Spaniards. In the second council held at Lima, in 1567, coca was described "as a worthless object, fitted for the misuse and superstition of the Indians;" and a royal decree of October 18, 1569, expressly declares that the notions entertained by the natives that coca gives them strength, is an "illusion of the devil" (*una elusion del demonio*). The Peruvian mine owners were the first to discover the importance of the *chacchar* in assisting the Indians to go through their excessive labour; and they, together with the plantation owners, became the most earnest defenders of coca. The consequence was, that, in defiance of royal and ecclesiastical ordinances, its use increased rather than diminished. One of the warmest advocates of the plant was the Jesuit, Don Antonio Julian, who, in a work entitled *Perla de America*, laments that coca is not introduced into Europe instead of tea and coffee. "It is," he observes, "melancholy to reflect that the poor of Europe cannot obtain this preservative against hunger and thirst; that our working people are not supported by this strengthening plant in their long-continued labours."* In the year 1793, Dr Don Pedro Nolasco Crespo pointed out, in a treatise, the important advantages that would be derived from the use of the coca plant, if introduced into the European navies; and he expresses a wish that experiments of its utility in that way would be tried. Though it is not probable that Dr Crespo's wish will ever be realised, yet there is little doubt that the use of coca, as a beverage on board ship, would be attended with very beneficial results. It would afford a nutritious refreshment to seamen in the exercise of their laborious duties, and would greatly assist in counteracting the unwholesome effects of salt provisions. As a stimulant, it would be far less injurious than ardent spirits, for which it might be substituted without fear of any of the evil consequences experienced by the *coqueros.* After a long and attentive observation of the effects of coca, I am fully convinced that its use, in moderation, is no way detrimental to health; and that, without it, the Peruvian Indian, with his spare diet, would be incapable of going through the labour which he now performs. The coca plant must be considered as a great blessing to Peru. It is an essential means of preserving the nationality of the Indians, and in some measure mitigating the melancholy fate of the once great race, which disease and excessive labour now threaten to destroy.

In former times, the cultivation of coca in the Montana de

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* The worthy Padre forgets the high price that would be charged for coca in Europe. In Turma and Huanuco, the aroba (twenty-five pounds) costs, at an average, six Spanish dollars; add to this the carriage to Lima, the freight to Europe, custom-house duties, &c., and this price would be nearly doubled.

Vitoc was very considerable. Upwards of 4000 arobas used to be annually forwarded to the market of Turma; now, only 50 arobas are sent.*

An Account of the Mining Academies of Saxony and Hungary. By Warrington W. Smyth, M.A., Cambridge, Mining Geologist to the Geological Survey of the United Kingdom. From a copy communicated by the Author.

Among the various applications of geology to the useful purposes of life, the art of mining most directly needs the application of that science, and requires, for the successful furtherance of its views, the most extended acquaintance with the geological facts observed at different times and in different places.

Every thinking miner must indeed, in a certain sense, though perhaps in a very confined sphere, become a geologist; little as the practical coal-viewer or mining agent may be prepared to receive the title;—and he bases his operations, whether relating to the discovery of new mineral treasures, or to the prosecution of those already found, on rules which the experience of years has taught him to lay down; and in proportion to the strength of his judgment, the retentiveness of his memory, and the opportunities of seeing and testing which have been afforded him, will he be likely to carry out advantageously the work on which he is engaged.

Looking at the class of men who, in this kingdom, are intrusted with the direction of collieries and mines, we find them, in general, characterised by a remarkable degree of energy and intelligence; and yet it cannot be denied that, independently of the losses entailed by the uncertainty of mineral veins, large sums are yearly squandered on ill-judged, and sometimes even absurd, speculations, which a greater amount of experience, on the part of the proposer, would have taught him to modify or abandon. We cannot be surprised to hear of similar failures, when we consider, first, the comparatively short time over which a single man can extend his experiences; secondly, the great amount of phenomena which must be observed and compared to form a groundwork for practical geology; and, thirdly, the numerous branches of other arts and sciences, some of which should properly precede its study, whilst others, immediately connected with the duties of the mining agent, intervene to distract his attention, and render it difficult for him to attain a great degree of proficiency in any one particular subject.

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* Travels in Peru. By J. J. Von Tschudi, well translated from the German, by Thomasina Ross, p. 447.

On the continent of Europe several academies have been founded, with the desirable object of supplying to miners a knowledge of geology, and the kindred sciences, and of establishing a storehouse of facts and experience, whence they should be enabled to reap the harvest sown by those who have gone before them, and should set forward on their own career provided with such a stock of knowledge as to obviate the necessity of passing a great portion of their lives in the useless repetition of that which has been done before; and with the view of affording them a facility, by beginning where others have ended,—to form new links in the chain of improvement, and thus to carry out the process by which alone, in every pursuit, we can hope to approach perfection.

To argue at length for the advantages of such a system would be here out of place; no man will deny the justice of the observation of the Roman poet—

> " Ego nec studium sine divite venâ,
> Nec rude quid possit video ingenium."

And a suitable direction of the mind, and early exercised acquaintance with established principles, is surely no less necessary to the successful cultivation of the useful arts, than to the ornamental branch of literature.

The most remarkable of these mining colleges are those of Freiberg in Saxony, and Schemnitz in Hungary, because both situated in the centre of important mining and metallurgical operations, and therefore combining the advantages of general scientific instruction with the practical application of the art. As no account, however, of their plan and working has been laid before the public since the days when the name of the first-mentioned academy resounded throughout Europe, in connection with that of its celebrated Professor, Werner, I believe a short description will prove interesting to the miner as well as the metallurgist and mine-adventurer; and since I had the opportunity of residing at each for some months in the years 1840 and 1842, and of hearing certain courses of lectures, as well as of examining the works of the surrounding districts, the information will apply nearly to the present date.

The want of an academy for educating the miners of Saxony was made apparent by the number of young men who flocked to Freiberg as pupils of Dr Henckel, and in his house heard lectures on all the necessary subjects; whilst in the neighbourhood they were provided with the means of gaining practical experience. On his death, in 1744, students still visited the town; and at length, in 1765, a mining school was founded by Prince Xaver, during the minority of the King, which very soon, under the care of Werner, who was appointed inspector in 1775, attained great celebrity. His perfect acquaintance with the sciences on which he lectured, his activity of mind, and his excellent manner of teaching, caused his class-rooms to be frequented by strangers from all quarters. He every year read lectures on the art of mining, mineralogy, and geology, as well as on iron smelting, fossil remains, mineralogical literature, geography, &c.

These subjects have, since his time, been so amplified as to afford sufficient occupation to several Professors; and the manner in which they were distributed in 1840 was as follows:—

General Chemistry . . .	Professor Lampadius. In 1844, Plattner.
Technical Chemistry . . .	
Metallurgy	
Mineralogy and Mineralogical Exercises	Professor Breithaupt.
Geology, in two courses . .	Professor Naumann, senior. In 1844, Cotta.
Crystallography	
Physics (Natural Philosophy) .	Professor Reich.
Palæontology	

Pure Mathematics	Professor Naumann, junior.
Higher Mathematics	
Mining Jurisprudence and Correspondence	Professor Lehmann.
Analytical Preparation	Professor Kersten.
Analytical Chemistry	
Applied Mathematics	Professor Weisbach.
Mining Machinery	
General Surveying and Practical Geometry	
Art of Mining	Professor Gätzschmann.
Surveying	Surveyor Leschner.
Drawing, in three divisions	Mr Heuchler.
Architecture	
Assaying of Metals	Mr Klemm.
French Language	Mr Munde.

The institution is placed under the immediate control of the upper board of mines (*Oberbergamt*); the Professors, although forming no regular senate, meet on occasion in conference, where the members of the above board also sometimes take a part. The superintendence of the discipline is intrusted specially to one Professor.

Candidates for admission must produce certificates of healthy constitution, good character, and tolerable proficiency in the usual subjects of school education, and in mathematics. The testimonials are presented between January and June; and the Oberbergamt decides upon the candidates who may present themselves for the examination, which takes place in August. Those who are supported by the government have the advantage of attending the lectures gratuitously, and receive certain *stipendia* or scholarships: they are divided into two classes; the first, called the *Beneficiaten*, pass through a course of four years, and become candidates for the royal service: the other is composed of those who enter for places not requiring more than one or two years of study, or who are unable to enter as regular pupils in consequence of a want of vacancies in the corps. Independently of these, are Saxons, who pay their own expenses; and foreigners. Some of the academists finally go to the University of Leipzig to study jurisprudence, which is necessary for those who are to be officers in the various mining towns of Saxony. Another part, who have distinguished themselves for intelligence and industry, are sent to travel at the expense of the government, or to visit foreign universities.

Saxons, in good circumstances, and foreigners, can attend the academy on production of certificates of good character, and of their stay being duly authorised. They are expected, however, to pay for the courses of lectures as follows :—

General Chemistry, Analytical Chemistry, Mineralogy, the Higher Mathematics, and Machinery, each	30 dollars* per annum.
Geology, Physics, Applied Mathematics, Pure Mathematics, Metallurgy, Technical Chemistry, Architecture	20 ,, ,,
Art of Mining (two parts), Elementary Mineralogy, Mining Jurisprudence and Correspondence, Theoretical Surveying	15 ,, ,,
Practical Surveying, Assaying, and Drawing,	according to circumstances.

The students of this class are entirely free from the superintendence of the officers of the academy, and only attend the examinations if they require certificates : their number is usually about twenty, of whom three or four are officers of the Russian Mining Engineers, sent by their government to reap the advantages of extended experience ; whilst others are from Spain, Brazil, the smaller German States, &c.

The course of lectures opens on the first Monday in October, and ends the last Saturday in July; the two vacation months, August and September, being very useful, as giving the opportunity of making excursions with a view to mining and metallurgical observation.

The subjects are taught as in our own Universities, by lectures illustrated by figures on the black board, by experiments, and by the inspection of specimens, according to the nature of the case. One day in the week (Monday) is left without lectures, to enable the students to visit the works, or take part in the operations.

The ordinary pupils are expected to keep a fair copy of

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[*] The Saxon dollar is equal to 3s sterling.

their notes, and are examined at least every month as well as at the close of the annual course of lectures, and are classified and rewarded according to the result.

On the expiration of the second year, the student determines whether he will attach himself more particularly to the mining or the metallurgical departments; and then pursues a course of study more especially adapted to his end. In the fourth year, practical exercises in both branches form the main feature of the course of education.

The Academic or Mining School, an establishment subordinate to the above, is intended partly to educate youths of a lower class for the situations of under-managers and viewers, —partly as a preparatory step to the Academy. The number of scholars is restricted to 40, and they are instructed in arithmetic, geometry, the art of mining, elementary mineralogy, German grammar, and drawing, the whole course being arranged with a view to the combination of practice with a correct knowledge of principles.

A handsome building, in the centre of the town, has been erected for the use of the academy; it contains several lecture-rooms, an excellent library, to which the students are liberally admitted: a room appropriated to mining and mechanical models, and extensive and well-arranged collections of mineralogical and geological specimens.

The advantages which Freiberg possesses for practical instruction are perhaps not surpassed by any mining town in the world, seated as it is upon the metalliferous gneiss, pierced by dykes of porphyry, it is within a walk of an extraordinary variety of geological formations. On the north-west we pass over the mica-slate, famous for the remarkable mines of Braunsdorf, succeeded by clay-slates and conglomerates of the so-called transition class, and by the singular coal-field of Haynichen. Farther west, we may visit the coal and rich iron ores of Zwikkau. On the south we reach the wild crest of the Erzgebirge or Ore-mountains, where are situated the curious tin mines of Altenberg and Zinnwald; whilst on the east, we may roam through the lovely valley of Tharandt, and investigate the relations of the coal measures, the new red sandstone, the green sand, and chalk marl, with the intrusive masses of porphyry and syenite.

The metalliferous veins, exhibiting very distinct characters, may be classed in several groups, and present every species of relation one to the other: whilst in a circle round the town, with a radius of about three miles, they give occasion to the working of nearly 100 mines, where about 200 shafts, 71,000 fathoms of adit or water gallery, and about 250,000 fathoms of level or gallery, exhibit every species of timbering and masonry as used in mining. In the neighbouring valley of the Mulde, the ores are daily roasted and smelted in twenty or thirty furnaces of various construction, and the beautiful process of amalgamation is always to be seen in action.

The Academy of Schemnitz was founded during the reign of the Empress Maria Theresa, about the year 1760, for the purpose of educating officers to superintend the mining and smelting works throughout the whole of the Austrian dominions. The extent of the provinces composing the monarchy, and the low state of education in the more distant portions, made it very necessary to place under the direction of well-instructed men the management of departments so various as metalliferous mines, salt works, collieries, iron works, smelting furnaces, and forests, all of which are in some places in the hands of the Crown. The number of students has of late years considerably increased, and comprises not only those who seek for government employment, but also many who are sent by individuals, or by mining companies, to pass through the whole routine of study, or to attend only some particular courses, according to the nature of the operations which it is intended they should conduct.

Independently of political motives, which influenced the choice, Schemnitz offers great advantages for the site of a mining college. The mines adjacent to the town and distributed through the surrounding district, produce in large quantity ores of gold, silver, copper, lead, antimony, and iron, and numerous smelting works are supplied with fuel by the thick forests of oak, pine, and beech which clothe the hills.

A great amount of machinery is set in motion by water carefully collected into reservoirs, and applied either to raise water and material from the shafts, or to effect the dressing of the ores; whilst the deep water-galleries, or adits, carried for miles under a lofty mountain ridge, stand pre-eminent for boldness of project and skill in execution.

The institution has been endowed with great liberality; for not only are the professors paid by the government, but such a sum is yearly granted for the expenses of the well-ordered laboratory, and for other experimental purposes, that the students obtain gratuitously an excellent education. To obtain admission, subjects of the Emperor must be able to produce certificates of having passed good examinations at their schools, and also a recommendation from one of the *Hofräthe* or Aulic councillors; whilst the same advantages are open to foreigners provided with a suitable introduction through the ambassador, or some known person in their own government.

The system of education must be followed out equally by all during the first year, after which a certain number who are allowed to pass aside to the *Forstwesen*, or forest department, are freed from a great portion of the after studies, whilst it is left to the choice of the rest to pursue voluntarily that branch in addition to the regular routine.

The subjects are divided as follows:—

First Year.

1st half.	Elementary Mathematics, consisting of Geometry and Algebra, Plane and Spherical Trigonometry, Conic Sections. Plan-drawing.
2d half.	Natural Philosophy (*Physik*) and Mechanics. Descriptive Geometry and Stereometric Drawing Crystallography.

Second Year.

1st half.	Chemistry. Mineralogy.
2d half.	Metallurgy (*Hüttenkunde*) Geognosy.

The Foresters' course is botany, practical geometry, &c.

Third Year.

1st half.	Suterranean Surveying (***Markscheidekunst***).
	Machinery and Drawing.
	Double Book-keeping.
	Botanical Abstract (on timber, &c.).
2d half.	Art of Mining (*Bergbaukunde*).
	Dressing of Ores (*Aufbereitung*).
	Architecture, or General Building.

The Foresters are exercised in valuing standing timber, &c.

Fourth Year.

Practical exercises. (*Verwendung*)	Surveying, common and subterranean,	4	months.
	Mining	1	,,
	Dressing and washing of Ores .	1	,,
	Smelting-furnaces. . . .	2	,,

During the third year, the young men are required on one day in every week to go through a portion of some mine, to examine minutely every particular, and to hand in a written report upon it to their professor. A few of them even take a piece of work on bargain, which assists to pay their expenses, whilst it gives them a practical acquaintance with sinking and driving.

The half-yearly examinations are conducted (notwithstanding that which some travellers may have stated to the contrary) with great strictness, and quite openly; and according to their result, the students are divided into those who gain preference (*Vorzug*), first class and second class; those who have the misfortune of being placed in the latter (*ein zweier Kriegen*) being obliged to undergo a second trial. The *Oberstkammergraf*, or supreme count of all the works in the district of Lower Hungary, also directs the academy, and in conjunction with the professors assists on these occasions. The questions are written on small slips of paper, and two or three of them are taken up at hazard by each in turn as he is called upon in alphabetical order, and are answered *vivâ voce.* The most successful candidates receive as a reward a species of exhibition, sufficient to defray a great portion of their expenses, whilst those who have been more than twice returned in the second class, must relinquish all claim to government employ.

In 1843 the number of students was between three and four hundred, and the Professors :—

Physics and Mathematics . .	Count Nyary
Chemistry and Metallurgy .	Dr Bachmann.
Mineralogy and Geology . .	Mr Petko.
Botany, and Management of Forests	Bergrath Feistmantel.
Art of Mining	Mr Landerer.
Subterranean Surveying . .	Surveyor Marschan.
Book-keeping	Bergrath Reuth.
Machinery	Mr Adriany.
Building and Plan-drawing .	Mr Hönig.

When the practical course has been brought to a conclusion, the passed students are draughted off to different places, and into the various branches, to serve for four, six, or even more years of probation upon a small pay, as *Praktikants*, after which, as vacancies occur, they receive the appointments for which they have been preparing.

The lectures are delivered in German, the language of the majority of the hearers, who come from Austria, Carinthia, Styria, Tyrol, and Bohemia. A tolerable library of works on technical subjects is open on appointed days, and books may be borrowed on certain conditions. The usual expenditure of the students is but small, varying from L.20 to L.60 a-year ; the underground mining suit is their usual dress, though a neat uniform is worn on great occasions ; a remarkable degree of harmony and good feeling prevails amongst them, and the duels so common in German universities are here unknown.

In looking at the system pursued at the above institutions, we find at both of them a prominent defect, tending to impair the usefulness of the class of men which they produce, viz., that the time is so short in which the scholars are expected to pay attention to such a multitude of subjects, that it is impossible they can acquire all of them soundly and practically ; and although an excellent foundation may have been laid, it must be left entirely to after years to rear up the superstructure, when, amid the pressure of business, and other cares and pursuits, there is only a small per-centage of men who feel an inclination to return to a system of study.

The mode of partially obviating this evil, would be to separate more decidedly the departments of the mine and the smelting-furnace; for although a portion of the earlier studies is necessary as a preparatory course for both, most of what follows cannot be obtained but at a great expenditure of time and labour; and in the ordinary routine it is as foreign to the business of the metallurgist to construct the timbering of a shaft, as to that of a miner to build a blast-furnace. There are some cases, it is true, where works are to be conducted in an uncivilized country, when such an extended knowledge must he highly desirable; but for these a fuller course of preparation is clearly necessary.

To Freiberg this remark is not so applicable as to Schemnitz, since in the former the two classes are kept more distinct; and it is probably to this source that we may trace the improvements,—more particularly in metallurgy, which emanate from the Saxon Academy. In Austria it has been too common to pass men from one department to the other, as if skill in the one point must necessarily indicate a good acquaintance with another; upon the same principle which, ere now, has elevated a good oarsman on the Golden Horn from his humble *cayeek* to the command of an army. On the other hand, I cannot but bear witness that the pupils of the Hungarian Academy, when full scope is given to them in the department which they have chiefly pursued, yield to none in the science and practical skill which they bring to bear on their object, as evinced particularly in some of their silver furnaces, in the water-pressure engines of Mr Adriany, and the improved dressing works lately erected on a large scale by Mr Rittengen, under the direction of the Oberstkammergraf von Svaiczer.

The nature of the metalliferous deposits in the two mining districts is so different, that it becomes difficult to institute a comparison between operations carried on necessarily in a very different manner to suit the character of the ground; and only so much is certain, that the system of each place is that best calculated to work to advantage its own lodes under their peculiar circumstances. At Freiberg we have a slightly undulated surface, hard gneiss rock, and mineral veins from two to six feet in width; so that in the mode of working, the small necessity for timber, and, in fact, in everything but the raising of water and of "stuff," we find there a perfect prototype of our own Cornish mines. At Schemnitz, on the contrary, the surface is roughly diversified with hill and dale, whilst gigantic lodes, of fifty to two hundred feet wide, sometimes forming with the decomposed felspar a mass of yielding clay, course along a ridge of greenstone porphyry, often softened by decomposition, and present difficulties to the miner which those alone can appreciate who have seen the driving of tunnels, or even small galleries, through such material. Shafts can rarely be sunk in the lodes, but are put down vertically on the hanging side; the common method of working out the contents cannot, from the great width, be adhered to, whilst the crushing softness of the filling substance demands unusual resources in walling and timbering.

Yet the scientific character of the Austrian miner has sunk below its former position, and below that which it ought from the above-named educational advantages to attain; and the cause, invisible to most on the spot, is evident to those who have examined works of a similar nature in other countries. A grave error has been committed in omitting the pursuit of science for that of practical art alone, and the consequent exclusion of the knowledge of advances made in other districts has led to their own state of backwardness. The varied circumstances of mineral deposit and other geological features occurring throughout the empire remain undescribed, and unreduced to their proper bearing, and the Schemnitz student generally passes to his distant charge, prepared only to conduct the works of the particular district around his academy, and expecting to see in other places a recurrence of the same phenomena. The very object of such an institution, to fit the miner by a general review of phenomena observed, and processes used, in various localities, to adopt what is most suitable to the particular locality in which he will be engaged,—is lost sight of, and he is left with the one-sided knowledge and attendant prejudices which in isloated mining countries have ever obstructed the way to improvement.

The little regard that has been paid to the advance of an important branch is evinced by the fact that, in 1841, the then Professor of Geology delivered, as lectures to the students, the unaltered notes he had made, nearly half a century ago, from the instructions of Werner! and that I have heard some of the first of his pupils revive the ancient dreams of the "plastic virtue" of nature in regard to fossils, and doubt the existence of a regular order of stratified deposits, because they had seen nothing of the kind immediately around them.*

The course of mining education fails, therefore, in one of its most important features, the inculcation of the general principles and the practical application of geology; and it need excite no surprise that serious blunders should often be committed, even in a country where such errors might be checked by the existence of an institution like the above, and, as too frequently happens nearer home, that coal should be absurdly sought in a non-carboniferous geological formations, where a few black shales attract the attention of the ignorant speculator.

It has been for some time expected that a complete change will be made in the Schemnitz Academy, and some are of opinion that the proposed end would be better attained if the preparatory studies were made in Vienna, and the observation and practice of the processes were left to be followed out in the mining district. There can be no doubt that both science and art must languish, if secluded from the general advance of other lands; and that if the cultivation of the former be forwarded in the stirring tide of full communication with the world, a valuable assistance will be conferred on those branches of the latter which are confined by nature to the bleak hill-side or remote mountain glen.

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* The individual above alluded to may have inserted his name in the Record of the Freiberg Academy, but could not have attended the Lectures of Werner. —*Edit.*

On the manner in which Sticklebacks (Cottoid fishes) construct their Nest and take care of their Eggs. By M. Coste.

M. Coste has collected some very curious facts relating to the instinct of sticklebacks, which shew that these fishes are susceptible of more variety in their acts than is generally supposed. We shall give the interesting communication entire :—

"I have placed in the College of France, and in circular basins about 2 metres in diameter and 33 centimetres in depth, where I brought together all the conditions which appeared to me material to the success of my experiments, a great number of male and female sticklebacks, caught at the time when they were about to deposit their eggs. A few days after they were put into their new dwelling I noticed certain males select a determinate point in the bottom of the basin as their permanent abode, and display a remarkable degree of activity. I endeavoured to observe what could be the object of all these manœuvres, and the cause of their constant return to the same place. I was not long in discovering that the occupation of each of them consisted in collecting materials for a construction which required all the resources of their industry; and by following with uninterrupted attention the rapid progress of their laborious enterprise, I witnessed one of the most curious spectacles that could be contemplated.

"I saw each of the males that was engaged in this work heap up, in the place he selected, pieces of grass of every kind, which he often brought from a great distance, seizing them with his mouth; and by means of these he began to form a kind of carpet. But as the materials which form the first part of his edifice might be carried away by the movements or oscillation of the water, he had the precaution to bring some sand, with which he filled his mouth, and deposit it on the nest in order to keep it in its place. Then, in order to make all the substances thus brought together adhere to each other, he pressed his belly against them, sliding slowly as if by a kind of vibratory creeping, and in this way glued them together by means of the mucus which exudes from his skin. By means of this operation the first collected materials form a kind of foundation or solid floor on which the rest of the edifice is to be reared: the execution of this he continues with a feverish perseverance and agitation. In order to satisfy himself that all the parts are sufficiently united, he agitates his pectoral fins with great rapidity, in in such a manner as to produce currents directed against the nest, and if he notice that the pieces of grass are moved, he presses them down with his snout, heaps sand upon them, flattens them, and glues them together again.

"When the process has reached this point he chooses more solid materials. We then see him seize small pieces of wood, sometimes straws, which he takes up with his mouth, and presses them into the thickness, or places them on the surface of the first construction. If he finds, when attempting to introduce them, that the position he has given them does not sufficiently answer his purpose, he draws them out again, seizes them at another part, again inserts them, pushes them forwards, until he judge that he has made the best possible use of them. Occasionally, however, in spite of all his care, there are parts which, owing to their shape, he cannot get to enter into the general plan of the edifice. These he draws out, carries to a distance from his nest and abandons; then proceeds to select others to supply their place. In this manner he finishes by hollowing out a solid bed, the different component parts of which he has always the precaution to solder together by means of the viscous matter which serves as glue.

"When he has succeeded in constructing in this way the floor and side-walls of his edifice, he then undertakes the roofing; and for this purpose he continues to carry materials similar to those used in laying the foundations. But while pursuing the completion of his enterprise with the most laborious activity, he does not neglect to secure at the same time its consolidation, and, with that view, he continually repeats the fatiguing manœuvre of the vibratory creeping, by means of which, as has been hinted, he agglutinates the various materials composing his nest. But in proportion as he exerts himself to consolidate his structure, it is necessary that he should arrange it suitably for the purposes for which it is intended. Accordingly he never fails to reserve an opening very neatly and regularly circumscribed, into which he often thrusts his head and even a large part of his body, in order to press out the walls and keep the interior cavity sufficiently dilated that the female may have sufficient room to lay her eggs in it: of these eggs he afterwards becomes the courageous and indefatigable defender.

When the construction of the nest is sufficiently advanced to receive the ova, the male darts forth, in a state of agitation, into the middle of a group of females, in order to attract the attention of one about to deposit her spawn and to offer her an asylum for her offspring. She easily distinguishes the ordinary males; for they wear the rich livery of love, and are adorned with the liveliest colour. Accordingly when she sees him advance, she becomes eager, goes up to him, slides along his back, and, by a series of coquettish movements, and mutual allurements, seems to express to him that she is ready to follow him. Then the male, understanding this mysterious language, hurries towards his nest, as if to point out the way, thrusts his head into the gaping aperture, eagerly enlarges it to facilitate her entrance, then gives place to the female, who, by entering, seems to accept his invitation, She enters within it, leaving nothing to be seen without but the extremity of her tail, which lies across the opening by which she entered. She remains there two or three minutes, during which her convulsive motions indicate the efforts she is making to extrude her eggs; then she darts out, pale and discoloured, after having pierced through the nest, so that, although it had only one opening before the entrance of the female, it has two after she has deposited her ova.

While she is engaged in this operation, the male, whose changing colours and animated motions express his increasing agitation, appears to be thrown into a kind of paroxysm, and seems desirous to hasten the moments when he may enter in his turn. He assists the female, rubs her with his snout, as if to encourage her, and when she has accomplished the painful operation of laying, he enters by the same opening that she followed, slides over the eggs with a rapid motion, and issues almost immediately in order to repair the disorders of his building. But the nest, whose construction has cost him so much fatigue, is destined to receive the eggs not of a single laying only; it can contain a much larger quantity. He therefore brings to it, at different times, and for several days together, either the same female or such others as consent to follow him. In general he fecundates the produce of each of these separately; and at last his nest becomes a rich magazine in which the ova of each particular laying are agglomerated in distinct masses, and all these together form a considerable mass.

"The male is the only guardian of this precious deposit, for not only do the females take no care of it, but they become its formidable enemies, forming part of those numerous coalitions which attempt to plunder it, and satisfy their voracious appetite by devouring the ova. It therefore becomes a very difficult task for him at once to defend them against these famished pirates or gourmands, and to supply them for a whole month with all the conditions favourable to their exclusion. But no obstacle can divert him from his purpose, nor daunt his courage. He first begins to strengthen his nest by covering it with stones, the size of which is sometimes equal to the half of his body, and which we would not believe him capable of transporting if we did not see him engaged in the task. However he always reserves one or more openings, which he enlarges as required, and through which he is often occupied in passing currents by the rapid motion of his pectoral fins. The use of these currents is probably to prevent byssus forming on the eggs and preventing their development, for these eggs all perish when not subjected to this salutary influence.

"While he is occupied in inducing the females to lay—in taking care of the eggs—in strengthening his nest against the attacks of the females—he drives away all the sticklebacks which approach, strikes them with his snout, threatens them with his spines, and as long as their numbers do not exceed four or five he always succeeds in repelling them by force. But there are times when the enemy becomes so formidable that all resistance is useless; and although, in this case, he is not able to defend himself, he does not renounce the hope of getting over the storm. He has recourse to a stratagem; leaves his nest with an unusually violent jerking movement, adopts all the lures of a fish pursuing its prey, and thus endeavours to effect a diversion. This stratagem often succeeds, for the sticklebacks, urged by the hope of wresting his prey from him, disperse without completing the act of spoliation they meditated against the eggs. But the artifice does not always preserve the nest from plunder; I have seen individuals obliged to attempt it five or six successive times.

"When he succeeds, by his assiduous care and courageous perseverance, in preserving his nest till near the time of hatching, his zeal is redoubled; he takes away the stones to give more easy access to the water, makes new openings and enlarges the old ones, multiplies the currents, moves the eggs, brings them sometimes to the surface, at other times carries them to the bottom, thus supplying them, by varying their position, with the conditions suited to this period of their development. Finally, when the eggs are hatched, he still continues to watch over them in his nest, and does not allow them to go at liberty till they have become sufficiently active to provide the means for their own preservation."

In the sitting of the Academy on the 1st of June, a letter was received without signature, stating that the Dictionary of Valmont de Bomare mentions circumstances analogous to those particularised by M. Coste. The writer of this letter ought in honesty to have added, that, in this dictionary, the only fact announced is, that the stickleback collects herbaceous substances in order to construct a nest (?) or a magazine of provisions (?), which it defends with vivacity against all the attacks made by other sticklebacks; and even this fact is referred to as doubtful.

M. Dumeril has presented to the Academy of Sciences (sitting of 17th Aug. 1846) a very favourable report respecting the observations made by M. Coste, and states that the commissioners have been able to verify their perfect accuracy.[*]

Fourteenth Letter on Glaciers. Addressed to Professor Jameson. By Professor J. D. Forbes. With a Plate.

On the Variation of the Motion at different Seasons.

Edinburgh, 12*th March* 1847.

My Dear Sir,—I am led to request you to reprint in the Edinburgh Philosophical Journal, part of the 9th section of my paper on the Viscous Theory of Glacier Motion from the Philosophical Transactions for 1846, for the following reason:—There has lately appeared in the *Bibliothèque Universelle de Genève*, a paper by M. Collomb, in which it is maintained, on the authority of the experiments of M. Dollfuss on the glacier of the Aar, that the velocity of progress of that glacier is the same at all seasons and in all weathers; and that previous observers who arrived at different conclusions on that glacier were in error. It may be so. The glacier of the Aar is, as I have elsewhere shewn,[†] in very peculiar conditions of mechanical constraint; but it is important that it should be shewn by the testimony of by far the completest series of observations which has yet been published, and which extends over the entire year, that this independence of the velocity upon external circumstances is not at least the *general* rule.

The passage in M. Collomb's paper, to which I have alluded, may be thus translated:—"It results from the examination of the registers of M. Dollfuss, which contain several thousand observations, that the glacier of the lower Aar moves with perfect regularity;—no sudden starts nor pauses; the movement of any particular point taken on the surface of the glacier, whether in the centre or at the edges, is very slow, uniform, and independent of atmospheric influences. [He goes on to say, that, as it is fastest in the centre of the glacier, and almost nothing at the edge], Fine weather or rain, dryness of the air or humidity, cold or heat, day or night, or different seasons, have no influence upon the velocity of any particular point of the surface of the glacier.

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[*] *Supplement à la Biblioth. Univ. de Genève.* No. 8, p. 437.

[†] Ninth Letter on glaciers, Ed. Phil. Jour., vol. xxxviii., p. 332. I may add, that M. Martins gives a similar account of the results of Mr Dollfuss's experiments in the *Comptes Rendus*, 26th October 1846.

"The observers of previous years believed, on the other hand, that the general march of great glaciers was in intimate connection with the accompanying state of the atmosphere. They believed, for instance, that the movement was retarded in dry and cold weather, and accelerated by moisture and rain. The observations of M. Dollfuss, which will, without doubt, be published one day in detail, have proved the contrary, and have shewn that *a glacier, like a semifluid body, urged by a mechanical force, moves on without being influenced by the state of the surrounding medium.*"*

Now, Sir, the last passage, the italics of which are in the original, conveys to us, in the first place, the pleasing information that M. Collomb, and probably M. Dollfuss also, have accepted the viscous theory of glacier motion. The word "semifluid" applied to a glacier, is now, notwithstanding its seeming harshness, an adopted word. But I must enter an earnest protest against the supposed discovery of the uniformity of the motion and its entire independence of atmospheric circumstances, being assumed to add any probability to the viscous theory, as the phraseology of the preceding extract seems to infer. On the contrary, if there be any glacier which does not present the law of variable velocity, which I established, for the first time, on the Mer de Glace of Chamouni in 1842, and which has since been found on the glacier of Bossons, on the glacier of Grindelwald, and was supposed to have been found on the lower glacier of the Aar, such a glacier affords *a proof the less* in favour of viscous or semifluid theory.

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* *Bibliothèque Universelle*, publiée 15 Decembre 1846, p. 212, note. The passage in italics stands thus in the original,—"*Un glacier comme un corps semifluide poussé par une force mécanique, marche en avant sans se laisser influencer par l'état du milieu ambiant.*"

It is on this account, Sir, that I desire that the readers of the scientific journals may be made fully aware of the amount of the evidence by which, in *some* glaciers at least, the direct connection between the movements of the glacier and the conditions of temperature and moisture have been established; and it is for this object that I crave a few pages of your Journal, for an extract from a more elaborate and less accessible paper.

Before quitting the subject, I wish to add, that I concur with M. Collomb in desiring the full publication of M. Dollfuss's results on the glacier of the Aar, which, latterly at least, I know to have been made with excellent instruments, and it may be hoped with due care. Until such a publication takes place, we must be permitted to withhold our final assent from a conclusion which is in contradiction to observations on at least three other glaciers, and to former observations on the same one. Should it prove correct in the particular case of the glacier of the Aar, I suspect that it can only arise from a curious balance of two opposing influences occasioned by the peculiar circumstances of restraint under which that glacier moves, and probably may apply to only a very limited portion of its surface.

But I repeat, we must wait for the observations. The public cannot but receive with distrust, reports of conclusions drawn from unpublished observations so repeatedly contradicting one another, as those which have been furnished by observers on this same glacier of the Aar. Not to go farther back, within *four* years we have had *four* positive statements of fact said to have been deduced from observation, three of which are irreconcileable. First, we were told that the glacier was absolutely quiescent in winter, and moved onwards during the summer months only.* It was next admitted, that in winter there is also motion, only the summer motion is greatly in excess.[*] Next year (1844), we were told that the variation of velocity is not confined to summer and winter, but is extended to every variety of meteorological condition, in fact, is left on the basis on which I had already placed it (as regarded the Mer de Glace of Chamouni), two years previously. In the middle of summer, but during cold and snowy weather, which lasted nine days, the velocity of the glacier of the Aar fell below the mean velocity of the same point for the entire year; whilst during the succeeding sixteen days of fine weather, the daily average increased in amount by exactly *one-half*, and rose considerably above the annual average.[†] Now (1846), we are desired to consider these opinions and deductions to be erroneous, as well as the measured distances ascertained by M. Wild, who has hitherto been believed to be a competent surveyor, and who had found the advance of the points of his triangulation on the glacier to be *more than one-half* more rapid during summer than in the remainder of the year.[‡] The opinion of MM. Dollfuss, Martins, and Collomb, is, that seasons and weather make no difference whatever on the motions of the glacier of the Aar!

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* Agassiz 1842.—"Ce que je puis annoncer positivement dès à présent, c'est que le glacier est immobile en hiver." Letter to M. Arago, dated from the glacier of the Aar, *Comptes Rendus*, 8th August 1842. Compare Edin. Phil. Journal 1842, vol. xxxiii., p. 253, 254.

[†] Agassiz, 1843.—" Le mouvement est beaucoup plus accéléré en été qu'en hiver." Bulletin de la Société des Sciences Naturelle de Neufchâtel, 8th Nov. 1843.

[†] *Comptes Rendus*, 9th Dec. 1844, p. 1301.—" L'avancement [journalier] était loin d'être uniforme, il variait considérablement suivant les conditions atmospheriques." Compare Ninth Letter, in this Journal.

[‡] *Bull. de la Soc. de Neufchâtel*, No. 1. From the results there mentioned, I give the comparative motion of seven points of the glacier during fifty-seven days of summer, and for the same length of time taken from the average of the remainder of the year.

Summer.	Other Seasons.
Feet.	Feet.
50·2	33·4
54·8	34·9
47·9	27·9
47·1	29·6
35·0	26·3
25·5	16·5
18·3	11·5

From these conflicting results, it is plain that there have been reckless assertions made, and also observations unworthy of confidence. It is very much to be desired for the credit of all the parties who have had a share in the experiments on the glacier of the Aar, that the source of these discordances should be investigated. I remain, my dear Sir, yours very truly,

JAMES D. FORBES.

To Professor JAMESON.

Observations on the Motion of the Glaciers of Chamouni at different seasons.

I am now enabled to present a view of the actual progress of two glaciers during every part of the year from direct observation. For these I am indebted to the intelligent and persevering zeal of my excellent guide and assistant at Chamouni, Auguste Balmat, of whose character I have had the pleasure of forming a more and more favourable estimate the longer I have been acquainted with him. To the long training of the laborious summer of 1842, when he assisted me, he adds the further experience derived from my visits in 1843 and 1844, in the latter of which especially he became familiar with the nice precautions requisite in conducting the most accurate measurements, and received instructions from me which rendered him perfectly competent to continue by himself the simpler kind of measurements which I have alone required of him. The extraordinary exertions which he used to obtain the winter motion of the block D 7, under the Montanvert, in 1842–43, have been noticed in my former publications. On one or two occasions, as I learned afterwards from himself, being unable to ascend the usual path to the Montanvert for fear of spring avalanches, he actually clambered with a companion up the rugged ascent from the source of the Arveiron, plunging continually up to the middle in snow, for no other purpose than to make the observation which I had requested of him; and it would be unjust not to mention at the same time the admirable, because rare, generosity, with which he positively refused for himself any share of the remuneration which I pressed upon him the following summer, as some recompense for the fatigues and dangers which he had braved to obtain for me this information. With such a person, my confidence in the observations which he has made at points much more accessible, and with the experience of some additional years, is complete. I do not mean that mistakes may not occur, or even that the measures may not be less exact than I might have taken myself; but from my knowledge of the man, I am nearly as confident in their *being faithfully reported*, *exactly as they were made*, as if I had done so myself.

With a view to lighten the labour as much as possible, I selected two stations on the *glacier of Bossons*, and desired Balmat to select two on the *Glacier des Bois* (the outlet of the *Mer de Glace* towards the valley of Chamouni); all these points being tolerably accessible at every season of the year.

The general method of observation was the following:—vertical holes were driven into the ice with a 4-foot blasting iron, at the points whose motion was to be determined; and these holes were renewed from time to time as the surface of the ice wasted. A staff of wood 5½ feet long, was stuck in each, which projected sufficiently above the snow (which never appears to have exceeded 2½ feet deep on the glacier), to make it visible at all seasons. During winter the staves were frozen into the ice, and the waste being small, the holes did not require renewal. Two marks are then made of a permanent kind on the rocks of the moraine, or two staves driven in, or a distant object on the farther side of the glacier was observed, so as to mark out sufficiently a line transverse to the glacier, the prolongation of which passes over the hole in the ice when first made; and the advance of the hole in the ice beyond this fixed visual line marks the progress of the glacier. The want of a theodolite is supplied by directing the eye past a plumb-line suspended over the fixed mark on the moraine nearest to the glacier, the eye of the observer being over the farthest mark. As the spaces moved over were in most cases considerable, an error of a few inches, or even a foot, is not important to the result. The progress was in every case determined by means of a line marked with *English* feet and inches, left by me at Chamouni on purpose.

The results were communicated to me regularly by letter at intervals of a few weeks during the whole year, and all questions asked and explanations required by me were answered by return of post.

Those who may look with suspicion upon observations made in a remote place by a peasant of the better class, though they may not partake of my security in the results from knowing the character of the individual, will, I believe, have their doubts removed by the internal evidence of this important series of observations, which even a philosopher could not have invented, and which, it will be seen, are confirmed by data of quite another kind over which the observer could have no control, I mean the Meteorological Registers of Geneva and St Bernard.

The following Table contains, in a condensed form, the results deduced from Balmat's observations at the four following stations:[*]—

Bois I. On the Glacier des Bois a little way below the Chapeau, at about *one-third* of the breadth of the glacier from its eastern bank.

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[*] This table is abridged from those in the Philosophical Transactions for 1846, pp. 183 and 184, where the days and hours of each observation are more accurately given, and the total motions. The days of observation are, in general, the same for all the stations, but not precisely so in a few instances.

Bois II. On the Glacier des Bois near its lowest extremity, behind the *Côte du Piget*, and near its centre.

Bossons I. Upper station on the Glacier des Bossons, some way above the plateau where the glacier is usually crossed; on the west side, near the moraine.

Bossons II. Near the lowest extremity on the Glacier of Bossons, where free from the moraine, on the western side.

INTERVALS OF OBSERVATION.	Mean Daily Motion in English Inches.			
	Bois, No. I.	Bois, No. II.	Bossons, No. I.	Bossons, No. II.
1844. Oct. 2 to Oct. 14, . .	32·0			14·0
Oct. 14 to Nov. 2, . .	27·8			17·0
Nov. 2 to Nov. 19, .	24·2			
Nov. 20 to Dec. 4, .	11·8		17·3	13·1
Dec. 4 to Jan. 7, . .	11·5	3·3	15·9	13·0
1845. Jan. 7 to Feb. 18, .	14·0	2·6	13·6	12·0
Feb. 18 to Mar. 18, .	17·0	3·0	15·4	12·8
Mar. 18 to April 17,	16·9	4·6	12·9	10·2
April 17 to May 17,	22·5	7·3	23·3	19·4
May 17 to May 31, .	37·0	8·8	42·9	30·2
May 31 to June 19, .	38·4	8·3	34·1	27·8
June 19 to July 4, . .	42·3	11·1	42·1	32·3
July 4 to July 18, . .	52·1	14·6	30·6	26·4
July 18 to Aug. 6, . .	49·0	11·9	28·8	21·4
Aug. 6 to Oct. 6, . .	35·7	9·9	20·6	16·5
Oct. 6 to Nov. 8. . .	36·4	9·8	19·4	5·9
Nov. 8 to Nov. 21, .	30·1	7·5	22·6	7·2

These four sets of observations are projected in Plate V., where the four lower zigzag curves represent the gradation of diurnal velocity by periods, according to the method adopted in projecting my own observations in my Travels, p. 141. The general accordance is sufficiently manifest, and the effect of the season of the year is beautifully shewn, the following being the minimum and maximum values:—

	Daily motion in inches.
Glacier des Bois, No. I., minimum in December, . .	11·5
Glacier des Bois, No. I., maximum in July, . . .	52·1
Ratio of maximum to minimum, . . .	4½ : 1

	Daily motion in inches.
Glacier des Bois, No. II., minimum in January, . .	2.6
Glacier des Bois, No. II., maximum in July, . . .	14·6
Ratio of maximum to minimum, . . .	5½ : 1
Glacier des Bossons, No. I., minimum in March, . .	12·9
Glacier des Bossons, No. I., maximum in May, . .	42·9
Ratio of maximum to minimum, . . .	3½ : 1
Glacier des Bossons, No. II., minimum in March, .	10.2
Glacier des Bossons, No. II., maximum in June, . .	32·3
Ratio of maximum to minimum, . . .	3¼ : 1

From these observations we may deduce the annual motion from November 1844 to November 1845 with considerable exactness. Allowing for the fractional parts of a year, we obtain the following results, amongst which I have included a separate computation of the mean daily motion for the summer period (April—October), and the winter period (October—April).

TABLE II.

	Bois, No. I.	Bois, No. II.	Bossons, No. I.	Bossons, No. II.
	Feet.	Feet.	Feet.	Feet.
Motion for 365 days, November 1844 to November 1845, . .	847·5	220·8	657·8	489·1
	Inches.	Inches.	Inches.	Inches.
Mean daily motion, ...	27·8	7·3	21·6	16·1
Mean daily motion, summer period, April to October, ...	37·7	9·9	28·0	22·2
Mean daily motion, winter period, October to April, ...	19·1	4·7	15·8	10·7
Ratio, summer : winter, motion, .	2·0 : 1	2·1 : 1	1·8 : 1	2·1 : 1

I. From this Table we deduce in the first place a mean annual motion far greater than has hitherto been observed, or perhaps suspected in any glacier, that of near 300 yards, or almost *one-sixth* of a mile. This is on the Glacier des Bois beneath the *Chapeau*, where the inclination of the glacier is very steep, adding a new illustration of the general principle,* that in *similar* circumstances the velocity increases with the slope. To this cause may be added the high temperature of the air of the valley to which in this part of its course it is exposed; but this last cause is alone insufficient; for

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* Travels, 2d Edit., p. 371.

II. We find that the lowest part of the same glacier immediately behind the Côte du Piget, a little way above the source of the Arveiron, and therefore still deeper in the valley, has a mean velocity nearly *four times less*, arising solely from the diminished slope and volume of the glacier in that part.* Hence there must be a condensation of the ice here, a pressure *à tergo*, the quicker moving ice pressing against the slower, consolidating it, remoulding its plastic material and sealing the crevasses; and a slight examination of the state of the glacier at the points in question will shew this to be the case.

III. All that has now been said with respect to the two stations on the Glacier des Bois may be repeated with only numerical differences with respect to the two stations on the Glacier des Bossons; the one set of observations confirming the other.

IV. In both glaciers the summer motion exceeds the winter motion in a greater proportion, as the station is lower, that is, exposed to more violent alternations of heat and cold; this we shall find to be general.

Before continuing our deductions, we would call attention to the close relation which may be established between the mean temperature of any portion of the year and the velocity of the glacier corresponding to it. This is done in Plate V., exactly in the same way as I did when comparing my observations in the summer of 1842 with the corresponding changes of temperature.† That is to say, I have projected by *periods* (corresponding to the intervals of observation on the glaciers) the mean temperatures as observed at Geneva and at the Great St Bernard, which are regularly published in the *Bibliothèque Universelle*, the average of which (separately deduced from the mean of daily maxima and minima, and projected in the upper part of the figure) may represent not inaptly the average temperature to which the glaciers in question, and especially the middle and lower regions of them, are exposed; and further, this average possesses the advantage of being derived from data wholly unconnected with the place or parties where and by whom the observations on the motion of the glaciers were made, and therefore are free from the remotest suspicion of either in any degree influencing the other.

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* This explains a circumstance which has always hitherto been a difficulty to me; the united testimony of the best informed inhabitants, not only at Chamouni but elsewhere (as at Zermatt and at the Simplon), to the effect that during winter the lowest end of a glacier, which terminates in a valley, does not greatly protrude, nor force the snow before it. This arises in fact from the comparative smallness of the motion which the *tongue* of such a glacier appears to possess, especially in winter.

† Travels, p. 141.

TABLE III.

Mean Temperatures (by periods) on the Centigrade Scale, observed at Geneva and the Great St Bernard.

	Means of Max. and Min.	
	Geneva.	St. Bernard.
1844. Oct. 2 to Oct. 14, . .	12°·97	1°·29
Oct. 14 to Nov. 2, . .	8·45	− 1·66
Nov. 2 to Nov. 19, .	7·47	− 2·02
Nov. 19 to Dec. 4, .	2·27	− 8·18
Dec. 4 to Jan. 7, . .	− 0·27	− 8·33
1845. Jan. 7 to Feb. 18, . .	− 1·32	−10·39
Feb. 18 to Mar. 18, .	1·01	− 8·24
Mar. 18 to April 17,	6·42	− 4·97
April 17 to May 17, .	10·80	− 0·60
May 17 to May 31, .	11·87	0·68
May 31 to June 19, .	18·20	6·29
June 19 to July 4, . .	17·35	5·17
July 4 to July 18, . .	18·42	6·14
July 18 to Aug. 6, . .	18·24	5·46
Aug. 6 to Oct. 8, . .	16·23	3·83
Oct. 8 to Nov. 8, . .	7·81	− 0·60
Nov. 8 to Nov. 21, .	9·20	− 3·54

A general comparison of the curves of temperature and those of glacier motion (more particularly on the Glacier des Bois) affords a proof of the justness of the principle laid down by me in 1842, that the motion of the ice "is more rapid in summer than in winter, in hot than in cold weather, and especially more rapid after rain, and less rapid in sudden frosts;"* the evidence of the connection is plainer by mere inspection than any detail could make it. But I request attention to the apparent anomalies of the curves, as affording a stronger evidence of the fidelity with which the measurements have been made, and to the truth of the plastic theory, than perhaps even the general coincidence just referred to.

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* Fourth Letter on Glaciers, Edinburgh Philosophical Journal, Jan. 1843; and Appendix to Travels, 2d Edit., p. 415.

If the velocity of the glacier depend upon the completeness of its infiltration with water, rendering the whole an imbibed porous mass like a sponge, it cannot depend solely on the mean temperature of any period, but also upon the *wetness* of the surface, whether derived from mild rain, from thawing snow, or from any other meteorological accident which the register of the thermometer cannot of itself indicate.* Further, a thick coating of snow on the glacier must defend it from the excessive cold of winter, just as it defends the earth and plants, and consequently the minimum of motion will not necessarily coincide with the minimum of temperature. Now, to estimate these more irregular causes is not easy; but some light is thrown upon them by a register of the weather and state of the snow, voluntarily kept for me at Chamouni by Auguste Balmat; which forms a valuable supplement to the thermometrical register of Geneva and St Bernard. Although the daily details would take up too much space, I will endeavour to give a faithful abstract of them so far as to give a general idea of the climate of Chamouni from October 1844 to November 1845. This diary includes (at my request) occasional notes on the state of the source of the Arveiron, which are of considerable interest.

Weather at Chamouni.

1844, *October*.—A good deal of rain during the month, which on the 10th and 16th fell as snow on the hills (nine inches at Montanvert), and subsequently to the latter day the glacier at the Montanvert was not clear of snow during the winter. 14th, Source of the Arveiron diminished to *one-fourth* (of the summer volume). Ice-vault more than half closed.

November.—Till 14th much rain and snow; fine with frost after. 20*th*, Source of the Arveiron very low; has not shifted its usual position.

December.—Weather generally fine throughout; cold most severe from 7th to 12th.

1845, *January*.—The weather continued splendid till the 20th; greatest cold from -2° to -5° Reaumur. 19th, The vault has disappeared at the source of the Arveiron. 20th, The first snow fell which lay at Chamouni, and continued from this day, attaining a depth of 1½ foot in February. Up to this time all the secondary heights, even the Breven and Flegère, were clear of snow, and the weather suitable for Chamois hunting. Occasional snow till the end of the month.

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* "The *proportion* of velocity does not follow the *proportion* of heat, because any cause, such as the melting of a coating of snow by a sudden thaw, as in the end of September 1842, produces the same effect as a great heat would do."—Travels, 2d Edit., p. 372.

February.—Snow at intervals all the month. 13th, Greatest cold of the season; thermometer −15° Reaum. followed by fine weather. 20th, Snow lies 2½ feet deep at the upper stations on the glaciers; 1½ foot at Chamouni. The arch of the source of the Arveiron has wholly disappeared, but the water issues at the usual places as in summer. The water is reduced to a small amount and may easily be stepped across. It is *still whitish and dirty,* though less so than in summer; *except* when a change of weather is threatened (when it is as dirty as in summer).[*] *Same date,* The glacier of Bossons has extended itself much. "On ne s'y reconnait presque plus." It is advancing towards the moraine of 1818; and the lower end is at least seventy feet high.

March 1st–3d, Mild with rain; 3d–13th, cold; 15th, *heavy rain.* Alternate rain and fine till the end of the month. 27th, Not half a foot of snow lying at Chamouni. The source of the Arveiron has not opened a vault. The quantity and muddiness of the water the same as at the last report.

April.—First week fine; second week cold with snow; changeable to the end of the month. 16th, Source of Arveiron has not much increased in water since the middle of March. In the end of April the snow first disappeared from the lower part of both glaciers.

May.—The first half of the month fine, with occasional snow; the second half changeable, with rain. 17th, The source of the Arveiron has increased three-fourths(*means probably in the ratio of four to one*) since the middle of April, and is dirty. The ice-vault is not yet formed. 26th, The Glacier des Bossons advances rapidly, and is crumbling into pyramids. The end of the glacier is, at least, eighty-five feet high, and advances considerably, particularly during the month of May; and widens greatly.

June.—A changeable and wet month; a very late season.[†] The snow did not entirely disappear from the Mer de Glace, opposite the Montanvert, till the beginning of July. 6th—7th, The vault opened at the source of the Arveiron. The quantity of water since the end of May is the usual summer supply.

July.—Commenced with warm weather. 5th, Thermometer 27° Reaum. The snow has disappeared from the ice opposite Montanvert, but some patches remain on the way to the Jardin. The Mer de Glace is much higher in level (about forty feet) than in former years, and the marks made in the rock at the *Angle* (in 1842) are all covered. The crevasses much the same as usual. The glacier of Bossons has also increased greatly, and appears to be approaching its old moraines. The register for the greater part of July has not come to hand.

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[*] This important remark proves that in the middle of winter a temporary rise of temperature of the air over the higher glacier regions (which is the precursor of bad weather) not only produces a thaw there, but finds the usual channels still open for transmitting the accumulated snow water.

[†] It will be seen from the temperature curves that the thermometer *fell* considerably in the latter part of June, both at Geneva and St Bernard.

August.—A very changeable rainy month. 8th or 9th, The arch at the source of the Arveiron fell in, and did not form again during the season.

September.—Also a changeable month. Rain twelve days.

October.—A very fine month. No rain mentioned after the 7th.

A careful examination of this interesting register will explain several of the apparently irregular inflections of the curves of glacier motion. Thus (to continue our general remarks, p. 186), we find,

V. At the upper station on the Glacier des Bois, the least velocity occurred in December, whilst, at the lower station (and at both of those on the Bossons), a minimum coinciding also with that of the temperature of the air took place in January. This coincides with the important fact noted in the preceding register, that the upper part of the Mer de Glace was covered with snow from the 16th of October, which only lay in the valley of Chamouni from the 20th of January; the snow screening the ice from the extremity of the cold.

VI. The comparative march of the two glaciers bears a remarkable relation to their positions and form. In the Bossons we detect at once the sudden transitions and seemingly capricious changes of a torrent; in the Mer de Glace, we have the stately and regulated flow of a river, in which the slighter variations are absorbed by the predominant inertia of a comparatively stable mass. Now, the glacier of Bossons is, as every one who has seen it knows, a mere icy torrent, "a frozen cataract," which descends in a continuous mass from the level of the Grand Plateau of Mont Blanc to that of the Valley of Chamouni with very little impediment, with no confining bulwarks of rock, no contracting straits; and throughout this great vertical height of at least 9000 feet, the angle of descent is very steep indeed for so vast a mass. On the other hand, though the part of the Mer de Glace, called the Glacier des Bois under the Chapeau, is very steep, its "*régime*" is regulated by the supply derived from the reservoir glacier above, and, precisely as in rivers of great magnitude and length of course, and of moderate declivity, it yields sluggishly to impulsive or retarding forces which are checked and opposed by the multitude of sinuosities, the embaying of the ice in rock-bound expansions of the channel, the struggle of its passage through defiles, and the enormous friction of its lower surface.

Yet, lest we might attribute the irregularities of the torrential glacier to causes quite local and uncertain, we find them reflected more or less distinctly in the movements of the neighbouring one. Thus the anomalous retardation in the end of March and beginning of April appears in three stations out of four, as does that in the first half of June, showing clearly that it is not an error of observation. It appears that the thaw of the winter's snow during the month of May, saturating the pores of the glacier with water, produced (as we know that a thaw always does) a sudden and violent march, especially of the more susceptible or torrential glacier. So completely had this sudden move forced on the glacier of Bossons, encumbered by the spring avalanches, and loaded with all the fragments and snow masses which had remained temporarily suspended during the winter months, that the lower part of the glacier (as we read in the memoranda to the register) advanced and widened greatly, to an extent which it had not done for many years past, and seemed to change its whole character; and in February a similar temporary increase of volume had taken place; "on ne s'y reconnait presque plus," writes Balmat; thus accounting for the particular accession of speed which appears in that month. In both cases, after the rapid march in February and in May, a reaction takes place; the material is deficient, the excessive pressure has been removed by the previous overflow, and a lull occurs in March and in June.

VII. These irregularities, such as they are, even should we fail in entirely explaining them, are, at least, not to be attributed entirely to errors of observation, since different observations (which it is to be recollected were sent to England in so rough a state, that they required to be reduced and computed before the variations of velocity could be deduced from them) agree amongst one another, and agree with the phenomena casually noted in the Meteorological Register. They are very trifling in the movement of the Glacier des Bois, which presents a curve of remarkable regularity, giving a minimum about the end of December, and a maximum in July. The coincidence with the curve of temperature is greater throughout than we could have expected, considering the important difference of circumstances which occur in autumn and in spring when the thermometer stands nearly alike, the first chill of autumn depriving the glacier of its fluid pressure more effectually than the severer cold of winter which is tempered by its snowy covering, whilst in spring the first relaxation of the bands of frost saturates the icy mass with the impetuous streams of melted snow, as effectually as the intensest heat of summer. In fact, the velocity would probably be greatest in spring, were it not that then the ice has attained its greatest consolidation by the slow but continued effect of the winter's cold penetrating its upper layers, though after all probably to no very great depth. But this is undoubtedly the reason why the minimum and maximum approach so near to one another in point of time in the torrential glacier of Bossons, and it receives an important illustration from the independent fact of the observed condition of the source of the Arveiron, which (see the Meteorological Register), though very small in February, was still whitish and dirty before a change of weather, shewing that the bands of frost were not so strong as to prevent a temporary relaxation of thaw throughout the mass of the glacier even in winter; and although the *mean* temperature of the air had been rising ever since the middle of January, and the greatest cold had occured early in February, we find that at the end of March the source of the Arveiron was still as small as in February, and that owing to the coldness of the spring, it had not even increased very much till the middle of April, when it almost suddenly resumed its summer volume. Now, during all this time the velocities of the glaciers underwent but little change,—some oscillations backwards and forwards,—but took no real start until the frost had given way, and the tumultuous course of the Arveiron shewed that its veins were again filled with the circulating medium to which the glacier, like the organic frame, owes its moving energy.

VIII. Being curious to see how far a relation might be established between the temperature of the air and the motion of the glacier, independent of the irregularly acting causes above adverted to, I projected the motions of the several points of the glaciers in terms of the temperature of the air for the periods already mentioned. It is to be recollected, however, that the observations of the thermometer were not made on the spot, and indeed it would have been difficult to have fixed upon a spot which should represent the mean circumstances of the whole glacier. Perhaps, therefore, the average of the observations at Geneva and St Bernard (the mean of whose elevations is 4750 English feet above the sea, and therefore between that of Montanvert and Chamouni) may represent pretty fairly the climateric conditions of the inferior parts of the Glaciers des Bois and Bossons. Now, if we examine the curves in question, they indicate *an almost perfect uniformity of motion until zero of the centigrade scale of temperature is reached;* but, the thawing point of ice past, the velocity manifestly goes on increasing with the temperature, in a ratio which would appear to be tolerably uniform if we neglect the irregular inflections of the curves.

IX. I am unwilling to multiply deductions which every intelligent reader will draw for himself; but one more I must add. It very clearly appears that the variations of velocity due to season are greatest where the variations of temperature of the air are greatest, as in the lower valleys; but it also appears from Remark VIII., that variations of temperature below 0° centigrade, or 32° Fahrenheit, produce almost inappreciable changes in the rate of motion of the ice. Hence, from this circumstance alone, we should deduce that in the higher parts of the glacier (where, for example, it freezes almost every night in summer) the variations of velocity should be least, and indeed comparatively small at different seasons. This is well illustrated by comparing the summer motions of the stations D, A, and C, mentioned in the first part of this section,* with their annual motion, which exhibit a much slighter excess in favour of the summer period than in the lower stations which we are now discussing. The same thing was observed by M. Agassiz's surveyors on the glacier of the Aar. Their position of observation far up on the glacier of the Aar, in a spot having a mean temperature near the freezing point if not lower, had a summer daily motion of 7·99 inches, and a mean daily motion during the whole year of 6·41 inches.† Now at station C, or the Pierre Platte, on the Mer de Glace, the mean motion for July 1842 was 10 inches, and for the whole year, 1842–43, it was 8·56 inches. It is quite evident that the motion of any point in the midst of a glacier is controlled by that of those which precede and follow it, and that it does not necessarily result, either that all must at once suffer a similar increase or diminution of speed, or that the times of maxima and minima, or even the general form of the annual curve, shall be the same.

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* See the Philosophical Transactions, 1846, p. 179. This part of the section is not reprinted here.

† Comptes Rendus, Dec. 9, 1844.

Long. 3° 12′ W., Lat. 55° 13′ N. Height above the Sea, 180 feet; Distance from the Sea, 10 miles. Rain-Gauge 5 feet from the Ground.
The Observations made at 9 A.M. and 9 P.M.

BAROMETER.

1846. Months.	Atmospheric Pressure, Morning.	Atmospheric Pressure, Evening.	Mean of Morning and Evening.	Reduced to 32° Fahr., and corrected to sea-level.	Mean Daily Range.	Mean Nightly Range.	Mean Range of 24 hours.	Monthly Extremes. Highest.	Monthly Extremes. Lowest.	Monthly Extremes. Greatest Range in 24 hours.	Monthly Extremes. Least Range in 24 hours.
January	29.49	29.51	29.50	29.64	0.151	0.136	0.287	30.39	28.60	0.61	0.04
February	29.70	29.77	29.73	29.87	0.090	0.094	0.184	30.31	28.98	0.50	0.01
March	29.85	29.83	29.84	29.98	0.120	0.100	0.220	30.41	28.78	0.83	0.02
April	29.74	29.69	29.71	29.83	0.118	0.093	0.211	30.15	28.93	0.43	0.02
May	29.80	29.81	29.80	29.91	0.078	0.057	0.135	30.20	28.90	0.53	0.01
June	29.74	29.73	29.73	29.83	0.110	0.088	0.198	30.25	29.22	0.48	0.01
July	29.46	29.74	29.60	29.80	0.074	0.081	0.155	30.08	29.02	0.46	0.00
August	29.69	29.62	29.65	29.75	0.081	0.076	0.157	30.24	29.39	0.33	0.01
September	29.75	29.73	29.74	29.84	0.100	0.109	0.209	30.40	29.22	0.39	0.01
October	29.75	29.75	29.75	29.87	0.101	0.101	0.202	30.12	28.70	0.87	0.08
November	29.46	29.44	29.45	29.51	0.114	0.142	0.256	30.40	28.70	0.82	0.01
December	29.51	29.50	29.51	29.65	0.251	0.200	0.451	30.35	28.80	0.49	0.03
Means	29.66	29.67	29.67	29.79	0.115	0.106					
1845	29.64	29.68	29.66	29.83	0.119	0.105					

THERMOMETER.

Months.	Mean of Greatest Heat.	Mean of Greatest Cold.	Mean Temp. of Morning.	Mean Temp. of Evening.	Mean of Extremes.	Mean of Morning and Evening.	Mean of Both.	Mean Range of 24 hours.	Monthly Extremes. Highest.	Monthly Extremes. Lowest.	Monthly Extremes. Greatest Range of 24 hours.	Monthly Extremes. Least Range of 24 hours.	Temp. of Spring-water.
January	47.2	39.3	42.1	43.4	43.2	42.7	43.0	7.6	51.5	30.0	18.5	3.5	44.1
February	48.8	39.0	42.2	43.7	43.9	42.9	43.4	9.9	56.5	29.5	17.0	3.5	43.0
March	48.5	36.9	41.5	42.0	42.7	41.7	42.2	11.4	54.5	22.5	17.0	3.5	42.8
April	52.5	39.0	46.4	44.3	45.7	45.5	45.6	13.7	60.0	31.5	22.5	6.0	45.7
May	60.3	44.5	54.1	51.7	52.4	52.9	52.6	15.8	71.0	36.0	28.0	6.0	49.5
June	72.3	52.6	65.0	63.0	62.4	64.0	63.2	19.8	83.0	43.0	31.5	8.0	53.3
July	65.0	54.1	59.1	58.8	59.5	58.9	59.2	10.6	81.0	46.0	31.0	0.0	54.0
August	68.2	54.4	61.4	61.0	61.3	61.2	61.2	13.7	78.5	45.0	25.0	4.0	49.0
September	65.8	57.5	57.9	57.5	61.6	57.7	59.6	13.7	74.5	40.5	26.0	6.0	
October	55.1	44.3	49.4	49.2	49.7	49.3	49.5	10.8	63.5	31.5	21.5	3.0	
November	49.4	41.4	43.6	43.2	45.4	43.4	44.4	8.0	57.0	27.0	18.5	2.5	
December	38.5	29.0	32.8	34.0	33.7	33.4	33.5	9.5	45.5	18.0	21.0	3.0	
Means	55.9	44.3	50.1	49.3	50.1	49.7	49.9	12.4					
1845	52.7	40.4	46.4	46.4	46.5	46.4	46.5	12.1					

Dr DUNBAR'S *Meteorological Observations for* 1846—(Continued).

WINDS—THEIR DIRECTION AND FORCE, AND WEATHER, IN THE NUMBER OF DAYS IN WHICH EACH PREVAILED.

MONTHS.	N.	NE.	E.	SE.	S.	SW.	W.	NW.	Calm.	Moderate.	Brisk.	Strong Breeze. Boist.	Stormy.	Sun Shone out.	Rain fell.	Snow or Hail.	Frost.	Thunder.	Rain in Inches.
January	1	...	3	3	6	16	2	...	12½	8½	...	9	1	16	23	1	2	...	3.24
February	3	...	...	...	3	4	11	7	7½	10½	1	8	1	23	16	1	5	...	1.52
March	1½	5	2	3	6½	8	4	1	10	12	1	6	2	29	19	6	11	1	3.12
April	1	12	6	5	...	2	3	1	4	11½	4	10½	...	25	20	H 2	8	1	1.94
May	...	½	5½	3	4	10	3	5	8½	8	4	10½	...	27	12	...	...	1	1.96
June	½	2	½	4	4½	13½	4	1	10½	3	8	8½	...	28	10	H 1	...	4	4.85
July	...	2	1	½	1	18	6½	2	10	9	8	4	...	27	21	...	...	2	5.79
August	1	6½	4½	5½	2	9½	2	...	18	9	3	...	1	27	15	...	...	4	4.85
September	...	7	7½	4	...	9½	2	...	16½	8	3½	3	...	27	10	...	...	3	1.25
October	1½	8½	1	9½	1	7	...	2	5	10	3	11	2	27	20	...	2	2	4.19
November	2	5	5	11	...	3	...	4	12	3	1	13	1	16	14	1	5	1	3.55
December	3½	4	1	...	...	1½	4	17	15½	7	1	7½	...	21	7	4	20	...	0.90
Total	15	52½	37	48½	28	102	41½	40	130	99½	37½	91	8	293	187	16	53	19	37.16
1845	25	57½	37½	37	31	80	49	48	110½	97	36	94	26½	302	162	20	86	6	30.32

REMARKS.

The maximum of *Atmospherical Pressure* was on the 9th of January, when the Mercury stood at 30·39. The minimum on the 22d of June, viz., 28.60. Range, 1.79.

The maximum *Temperature* was on the 17th and 18th of June, viz., 83.0; in 1845 it was 75½. Minimum, 14th December, viz., 18°; in 1845 it was 7°. The mean of extremes exceeds the mean of morning and evening by nearly 2½ degrees. The mean temperature of the year is 49.9, exceeding that of last year and of the annual mean for 25 years by nearly 3½ degrees. Yet the cold in December was very severe; the temperature of the month being 6 degrees lower than that of the same month in the preceding year. The great and sudden variations of temperature which took place in September 1845, and to which some were disposed to attribute the potato malady, were not experienced this year (1846), yet the failure was to a still greater extent.

The quantity of *Rain* is 3 inches more than the mean in this locality; and the number of days in which it fell, 25 more than in 1845. Total quantity 37.16 inches, 20½ of which fell during the latter half of the year. Rivers in frequent flood.

Compared with last winter, the *Frost* was severe in December, though, upon the whole year, the days in which it prevailed were, as stated in the Table, one-third fewer in number than in 1845. It is to be hoped the beneficial effects of the increased severity on the soil and atmosphere will be seen in the soundness of the ensuing crops.

Slight falls of *Snow* in this locality; surprisingly slight, considering how heavily it has fallen in other quarters at no very great distance; never more than half an inch in depth; a subject of some regret to our farmers on account of the young wheats.

The *Winds* have been moderate upon the whole; the calm days exceeding those of 1845 by 20; and the stormy days fewer by 18.

Synopsis of Meteorological Observations, made at Whitehaven in the Year 1846.

1846.	BAROMETER.							THERMOMETER.				RAIN GAUGE.		Wet Days.	Prevailing Winds.
	Max. Inches.	Min. Inches.	Mean at 9 A.M. Inches.	Mean at 3 P.M. Inches.	Mean at 9 P.M. Inches.	Mean of the three.	Range. Inches.	Max.	Min.	Mean.	Range.	Rain, &c., Inches.	Snow.		
January .	30·35	28·63	29·494	29·495	29·502	29·497	1·72	50°·5	32°·	43°·422	18°·5	4·604	...	22	SW.
February	30·26	29·03	29·741	29·743	29·752	29·745	1·23	56·5	27·5	43·928	29·	2·007	...	15	S. & SW.
March .	30·40	28·80	29·528	29·528	29·529	29·528	1·60	53·	25·	43·277	28·	4·460	·745	18	SW.
April . .	30·09	28·91	29·559	29·557	29·594	29·570	1·18	60·	33·5	46·525	26·5	2·848	...	17	NE.
May . .	30·16	28·86	29·756	29·748	29·740	29·748	1·30	64·5	40·	53·215	24·5	2·317	...	12	SW.
June . .	30·21	29·27	29·834	29·833	29·821	29·829	·94	83·	47·5	64·100	35·5	2·311	...	9	SW., Var.
July . .	30·09	28·94	29·680	29·684	29·693	29·685	1·15	84·	51·5	61·435	32·5	9·061	...	24	SW.
August .	30·17	29·32	29·783	29·773	29·787	29·781	·85	81·	49·	63·177	32·	4·066	...	13	NE. & NW.
September	30·34	29·22	29·823	29·818	29·824	29·821	1·12	76·	42·5	59·198	33·5	2·857	...	12	SE., Var.
October .	30·10	28·63	29·410	29·412	29·436	29·419	1·40	64·	37·5	51·249	26·5	7·982	...	23	SW.
November	30·33	28·45	29·723	29·698	29·724	29·715	1·88	56·5	26·5	45·261	30·	4·671	·034	18	SW.
December	30·33	28·81	29·680	29·682	29·695	29·685	1·52	49·5	21·	35·515	28·5	1·950	·357	17	NE.
1846,	30·23	28·90	29·668	29·664	29·675	29·668	1·95	64·8	36·1	50·858	63·	49·134	1·136	200	SW.
1845,	30·18	29·00	29·701	29·693	29·700	29·698	1·78	59·7	32·8	47·497	56·	49·207	1·299	...	S. & SW.
1844,	30·17	29·11	29·743	29·740	29·749	29·743	1·72	61·3	34·4	48·117	57·	36·723	2·240	...	NW.
1843,	30·14	29·01	29·706	29·702	29·708	29·706	2·42	61·3	34·5	48·986	56·	46·206	1·030	...	SW.
1842,	30·23	28·98	29·770	29·764	29·771	29·768	1·87	62·	36·7	49·837	50·5	34·693	·747	...	S.

Remarks on the Climate of 1846.

However great may be the fluctuations of temperature in the same months and seasons—however sultry the summer, or cold the winter, in any particular year, its mean temperature varies but little from the climatic, or average actual mean of the locality, when once correctly ascertained; and, even the greatest variation between one year, and any other the most opposite in character, and extending over a long period of time, when accurately expressed in figures, appears so trivial, that except to the meteorologist, it fails to convey any adequate idea of the excess or deficiency of heat, or of the absolute difference in temperature between the periods in question.

The years 1845 and 1846 present the opposite extremes of the mean temperature of this place; for, whilst 1845 is the coldest, its successor is the warmest of the last fourteen years. And, should the register be continued till half a century is completed, it is probable that the difference between the coldest and the mildest year in that period, will not greatly exceed the variation in temperature between the years 1845 and 1846, viz., 3·°36, or 3½ degrees per diem nearly. The mean temperature is 2·°17 above the average of the preceding thirteen years. This great increase is not solely occasioned by the excessive heat of the summer, for every month (December excepted) is considerably above the average temperature, as may be gathered from the following monthly remarks:—

January.—The mildest January on record at this place, except that of 1834, which exceeded it ¾ of a degree. The mean temperature is 5.°044 above the average, and on one night only has the thermometer descended to the freezing point. The complement of the dew-point is 1·°72 under the mean for January, and there has been no month in the last four years at all approaching to it in dampness, the point of saturation being only 1·48 below the simultaneous temperature of the air.

Among the many wonders of the season, a robin's nest, containing four eggs, is said to have been found near Warwick, Carlisle, towards the latter end of the month.

February.—The mildest and dampest February since I have kept a record, the years 1833 and 1834 approaching the nearest to it in temperature. The mean temperature is 4·°91 above the average, and the complement of the dew-point is less than in any other month of the past four years, except in January last. The atmosphere has been very unfavourable for free radiation of heat, both in January and February, there being scarcely an entirely clear night in either month.

On the 12th, perfectly formed gooseberries were gathered in this vicinity; and on the 13th, a tortoise-shell butterfly was caught at Keswick. On the 16th, I find the following entry in my register:—"The bees in our garden were this day actually bearing burdens, a circumstance probably without a parallel in the annals of the apiary. Bees were also noticed at work to-day at Lamplugh. There are now quantities of primroses to be found blooming on sheltered banks, and under warm hedgerows. This morning, 15th, I gathered fifteen healthy full-bloom flowers from a single primrose plant, in one of the lanes leading to Ennerdale Lake, leaving about as many more which were not fully expanded. Some of the flowers had bloomed and withered, and others seemed to have been out for a week or more. Primroses were gathered in Lamplugh on Candlemas day. The thrush and blackbird, with many of the feathered tribe, may be daily heard tuning their notes in concert, to welcome the approach of an early spring. The precocity of the vegetable kingdom is most remarkable, and very striking to the eye. Grass, in particular, has grown considerably of late, and the pastures look more green and healthy than we have seen them do in the middle of May, in some cold and backward seasons. The elder and woodbine, in sheltered spots, are nearly in full leaf. Near Cockermouth the wild gooseberry bushes are in leaf; in the gardens they are in flower, and in some situations they may be seen bearing fruit. On the 19th, the note of the cuckoo was distinctly heard in the grounds of William Knowles, Esq. of Sandford House, Bootle, Liverpool. A Liverpool gentleman informs me that he saw a swallow at Aigburth on the 28th instant, and another at the same place on the 2d of March. The cuckoo heard, and the swallow seen in the month of February! truly, this is a most extraordinary season. A scientific friend in a letter dated the 13th, states that a thrush's nest, containing eggs, was lately found at Rawden, near Leeds.

March, although colder than either of the preceding months, is 2·°687 above the average temperature. Severe frosts on the nights of the 18th, 19th, and 20th; also a fall of snow on the 21st, which covered the ground to the average depth of about 4½ inches.

On the 21st, and also on the 22d, the cuckoo was distinctly heard in the neighbourhood of St Bees, by an inhabitant of that village, the temperature being lower than it has been at any period during the winter. Between the 22d and 28th, its note was heard in various parts of the county. On the 28th, pear tree blossoms was fully expanded in this neighourhood. On the night of the 3d, a dreadful hurricane occurred at Glasgow; and at 9 A. M. of the 4th, the anemometer at the Observatory indicated a pressure of 45 lb. on the square foot, or 15 lb. more than the force of the memorable 7th January 1839. At Whitehaven, the pressure on the 2d was 8·68 lb.; on the 3d and 4th it was not noted; but by far the heaviest gale we had during the month, or probably through the winter, was on the 16th, when the horizontal force of the wind by Lind's anemometer, was 21¼ lb. on the square foot. A number of dreadful thunder storms occurred in various parts of the kingdom between the 24th and 31st, attended with great loss of life.

April.—Temperature 0·°17 above average. On the 10th, the Io butterfly began to appear. A swallow was seen at Braystones about the 8th, but they did not generally make their appearance till the 22d. Heard the corncrake on the 24th, being at least a week earlier than usual.

May.—Mean temperature ·14 above the average. From 9 A.M. to 3 or 4 P. M. of the 7th, there was the most magnificent solar halo I ever beheld. It extended over a space of 49° or 50°; at 1h. 45m., altitude of upper edge 74°; under edge 25°; the sun being 49° in altitute, and consequently nearly in the centre of the ring. The prismatic colours were very distinct and brilliant from 11 to 3 or 4 P.M., and most so towards the upper or northern portion of the circle. The phenomenon was formed through a thin veil of cirrus which prevailed in the SW.

June.—This is by far the hottest month in the last thirteen years and the mean temperature is probably higher than that of any June on record. The mean temperature of the first twenty-two days is 66·°06; the mean of the maximum being 74·°54, and of the minimum 57·°59. The mean temperature of the month is 5·°768 above the average. The mean temperature of June 1842, which approaches the nearest to it, was 61°, or 3° under that of the past month. The thermometer has attained to 80° and upwards, in two years only of the last thirteen; viz., on the 30th and 31st July 1834, it rose to 82·°5; on the 1st and 2d of August in the same year, it attained to 81·°5; and on the 12th September 1841, it reached 84°. On two days of this month the thermometer was at 80·°5; and on the 22d it rose to 83°. At Keswick and Cockermouth it attained to 86°; and at Carlisle to 87°. Notwithstanding the severity of the heat and drought, the atmosphere has by no means been remarkable in point of hygroscopic dryness; the complement of the dew-point being only ·92, or barely 1°, above the average of the last four years. The evaporation, which was greatly retarded by the motionless state of the air, is 2·555 inches above the mean quantity. Had a brisk breeze accompanied the heat, the evaporation would have been enormous. Copious dews were deposited at night, during the first fortnight in the month.

July.—The mean is 1·°080 above the average, and the quantity of rain is the greatest we have had in any month since January 1834. The evaporation is ·908, or 1 inch nearly, under the mean.

On the afternoon of the 5th inst., this county was visited by a heavy thunder storm, accompanied with hailstones of an almost incredible size. At Cleator some of the hailstones measured ⅝th of an inch in diameter, or 2 inches round, nearly ten minutes after they had fallen. At Distington and Gilgarron some of them were 3 inches in circumference. In Lamplugh, many of the hailstones were as large as an ordinary plumb, and, on examination, were found to be invested with a thin covering of snow, but within was a piece of ice, hard and clear as crystal. In Bassenthwaite the hailstones are described as the size of an ordinary pigeon's egg, and many of them not less than 3 or 4 inches in circumference. Mr Harvey of the Pheasant Inn, Peel Wyke, had no less than 20 squares of glass broken in the windows of his house. When it is considered that the sky was overcast with clouds from an early hour in the morning, I look upon this as by far the hottest day which has occurred within my recollection. The maximum here was 84°, but had the atmosphere been bright and clear, I quite think the thermometer would have attained to 90°. At 1 P.M. the thermometer stood at 84°, and at 5 P.M. at 63°, the temperature having fallen 21° in four hours. The fall of rain between 2 P.M. of the 5th, and 2 P.M. of the 6th, was 2·672 inches. A most extensive and destructive thunder storm occurred on the 5th July 1843, and there were extraordinary movements of the sea at Penzance, on both occasions. Another awful storm occurred on the 29th. The fall of rain in twenty-four hours at Whitehaven, was upwards of 2 inches. The floods in Lamplugh and Ennerdale were greater than any remembered by the oldest residents.

August.—This is the warmest August in the last thirteen years, and its mean temperature exceeds the average of this period by 3·°63. The evaporation exceeds the average by ·850. We have been favoured with one of the earliest and shortest harvests on record. Reaping commenced in this neighbourhood about the 2d, and by the close of the month, the crops were safely under cover. Even in the backward mountain districts, most farmers had secured their grain before the middle of September. The wheat crop is abundant, and of excellent quality; but oats and barley are both deficient. On the 1st, the metropolis was visited by one of the most tremendous and destructive storms of thunder and lightning on record in this country. (*For particulars, see illustrated London News.*)

September.—Mean temperature 3·°57 above the average; evaporation ·47 under the average. In consequence of the deficiency in the oat crop, oatmeal is now selling at 2s. 8d. per stone, the price of the best flour. Apples and pears are both deficient in quantity, probably owing to the precocious appearance of the blossom, which was partially blighted by a few sharp frosty nights in March. Many fruit trees blossomed a second time in this month, and there are some instances of their bearing a second crop of fruit. Swallows disappeared much earlier than usual, which is extraordinary, considering the mildness of the season. I did not observe any in this neighbourhood after the 18th. The magnificent meteor seen at London and Cambridge on the evening of the 25th, was also witnessed in the east of this county.

October.—Mean temperature 1·°30 above the average; the evaporation is 0·41 inch, and the complement of the dew-point is 0·°46 under the average. The quantity of rain is the greatest which has fallen in any one month, except July last, since January 1834.

November.—Mean temperature 1·°405 above the average. On the 28th, we had a slight fall of snow which yielded ·034 of water. During a heavy gale on the 20th, the barometer fell 0·97 in seventeen hours, and rose ·48 in the succeeding seven honrs, making a fluctuation of 1·45 in twenty-four consecutive hours. On the 30th, a swallow was seen on the wing at Parton. It has been observed daily by the inhabitants, for some time past, so that it has probably taken up its abode in that vicinity for the winter. On the 5th, saw a small brown butterfly in Lamplugh.

December.—Frost set in on the night of the 27th of November, and it continued with slight intermissions till near the close of the year. The mean temperatnre of December is 6°·05 under the average, and it is the only month in 1846 which has not exceeded its mean heat. It is the coldest December I have recorded, except in 1844, which had a mean temperature only 0·81 above the freezing point of water, being 2·°70 under the past month. The evaporation during the severe frost from the 10th to the 16th, is remarkably great; the average loss from the solid mass of ice in the gauge, is ·075 inch per diem, or more than double the average daily quantity for the whole month. A brisk breeze prevailed during the period.

I do not remember any winter in which I have seen such multitudes of meteors and falling stars, as in the present. From the 17th of October to the 17th December, they appeared in great numbers almost every clear night. Some were as large as the planet Jupiter, and such was their brightness, that for a second or so, they illuminated the whole sky. The tracks of many might be followed by trains of bluish light extending for some distance behind them; others exactly resembled rockets in appearance, and exploded in a shower of brilliant scintillations. The most remarkable of the meteors were observed between the 17th and 26th of October, and on the 10th, 11th, and 12th of November.

The oppressive heat of the summer of 1846 will long be remembered. The mean temperature of June, July, and August, is 3·°49 above the average, and is 5·°27 above the corresponding quarter in 1845. But, for convenience of comparison with the Registrar-General's report on the mortality, we have made the summer quarter to end with September, which reduces the excess of heat to 2·°96, or 3° nearly. This unnaturally high temperature has apparently had an injurious effect on the health, both of the animal and vegetable kingdoms. The number of deaths in the summer quarter (June 30th to September 30th) of the last eight years, in this town, and in the whole union, are as follow:—

	1839	1840	1841	1842	1843	1844	1845	1846
Whitehaven	66	40	64	80	60	61	54	200
Do. Union.	134	120	127	149	131	136	150	352

Hence, the deaths occurring in Whitehaven alone, in the summer quarter of 1846, exceed the average of the preceding seven years by 140, or 233 per cent.; they are nearly four-fold the number in 1845, and exactly five-fold the number in 1840. It is only fair, however, to state, that so far as Whitehaven is considered *per se*, a large proportion of the deaths were those of children who were carried off by measles, which made its appearance as an epidemic during this period. If, therefore, we take in the whole Union, comprising twenty-three districts, it will perhaps afford a more correct comparison. The average number of deaths in the quarter for seven years, is 135, and the greatest number is in 1845, viz., 150; but in 1846, the deaths are 352, shewing an increase of more than 160 per cent. over the average. The mortality throughout the kingdom in the summer quarter, was dreadfully great; 51,235 deaths were registered, a number greater by 15,227 than in the corresponding quarter of 1845, and 11,898 more than in any September quarter since the register was commenced in 1838. We may readily conceive how, in a summer like the past, marked by excessive heat, and a damp and stagnant condition of the atmosphere, and consequently favouring the rapid decomposition of all kinds of animal matter, those diseases referable to an impure atmosphere, as typhoid fever, &c., should exhibit an alarming increase; whilst cholera, diarrhœa, and other intestinal complaints, may have been favoured by a diet chiefly vegetable, and by profuse perspirations suddenly checked.

The potato disease had made its appearance generally throughout the kingdom by the end of July. In the report for last year, we endeavoured to shew that it could have little or no connection with atmospheric peculiarities, and we have seen nothing since which has induced us to alter that opinion.

Several strange birds and insects were observed this summer, having robably been allured from other climes by an instinctive sense of the nusual warmth of the season in higher latitudes. Small flights of lo-usts made their appearance in some counties, and a few specimens were ound in Cumberland. The nightingale is said to have been heard in his vicinity.

Hygrometer.—An inspection of the annexed table will shew the hygroscopic condition of the atmosphere in 1846, as compared with the four previous years :—

1846.	Mean Temperature 9 A.M.	Mean Dew-Point 9 A.M.	Mean complement of Dew-Point.	Differ. of Wet and Dry Bulb Thers.	Evaporation per hour in Grains.	EVAPORATION GAUGE. INCHES.
January .	42°·62	41°·57	1°·48	2°·25	4́·20̀	1́·01̀5
February .	43·03	40·69	2·33	2·62	7·80	1·335
March .	42·67	38·83	3·83	3·24	11·40	2·085
April . .	47·43	41·80	5·63	4·61	16·20	2·575
May . .	54·54	47·45	7·09	5·77	25·80	4·375
June . .	64·91	55·86	9·18	6·76	47·40	6·645
July . .	61·16	56·16	5·00	4·68	25·80	3·450
August .	63·46	59·44	5·13	3·65	28·80	3·875
September	59·30	54·23	5·07	4·25	21·60	2·980
October .	51·19	47.16	4·03	3·64	16·80	1·780
November	44·86	41·03	3·83	3·28	12·60	1·360
December	35·46	30·83	4·62	2·84	9·60	1·025
Means, &c.	50·88	46·25	4·76	3·96	19·00	32·500
1845 . .	47·72	42·56	5·18	...	19·80	32·432
1844 . .	48·27	42·53	5·66	...	20·80	31·719
1843 . .	49·17	43·60	5·33	...	20·36	26·981
1842 . .	49·88	43·50	6·14	...	25·80	...

The driest day in 1846 was the 22d of June. At 9 A.M. the interior thermometer of Daniell's hygrometer was depressed to 45°, or 26·°5 below the temperature of the air, without obtaining any deposition on the dark ball; at 5 P.M. the dew-point was 63°, having risen 18°, at least, since morning. There was much thunder, lightning, and rain, between five and half-past six the same evening, and on the following morning nearly an inch of rain was measured. This sudden change, after a drought of four weeks, confirms the observation of some meteorologists, that an extremely dry state of the atmosphere is sometimes followed by rain within a few hours. The evaporation indicated by the guage, is within one-tenth of an inch of the quantity in the previous year, and three quarters of an inch more than in 1844. The year 1846 has been the dampest of the last five years.

Terrestrial Radiation.

1845.	ABSOLUTE MINIMA.			TERRESTRIAL RADIATION.			
	Six at 4 feet.	Naked on Grass-plot.	Difference.	Max.	Day.	Min.	Mean.
January .	32°·	21°·4	10°6	14°·3	16	0°·	4°·50
February	27·5	12·5	15·	15·0	10	0·5	5·78
March .	25·	8·0	17·	17·5	21	0·	8·12
April . .	33·5	23·	10·5	13·0	29	2·0	6·90
May . .	40·	24.	16·	16·0	15	3·	8·52
June . .	47·5	33·5	14·	16·5	1	0·	9·07
July . .	51·5	43·0	8·5	12·5	5	0·	4·76
August .	49·	37·	12·	14·0	27	3·	7·02
September	42·5	32·3	10·2	13·0	10	3·3	8·16
October .	37·5	25·5	12·0	13·2	27	0·	7·49
November	26·5	11·3	15·5	15·5	16	1·5	7·46
December	21·	6·0	15·0	15·	14	3·	8·61
Means, .	36·12	23·12	13·00	14·62	...	1·35	7·20
1845, . .	32·87	22·06	10·80	12·75	...	...	...
1844, . .	34·45	27·	7·45	9·62	...	...	...
1843, . .	...	...	4·55	7·40	...	...	...

The direct effect of terrestrial radiant heat is determined by exposing a delicate naked thermometer on a grass-plot, and comparing its minimum with that of a Six's thermometer at four feet above the ground. The difference is the amount or effect of the radiation.

The first part of the above table needs no explanation. The first column under the head " Radiation," shews the *greatest* difference between the minima of the naked and standard thermometer, under the conditions above described; the second column indicates the day of the month on which it occurred; the next gives the *least* difference during the month; and the last exhibits the mean radiation, or sum of all the nightly differences during the month, divided by their number. Thus, the naked thermometer in 1846, one night with another throughout the year, has fallen 7¼ degrees lower than the standard thermometer at four feet.

The naked thermometer is generally exposed on a flat piece of cork about an inch in thickness, but in clear and settled weather it is placed in a wicker basket containing a layer of raw wool. Under a clear sky, the difference between a thermometer on cork and one on wool, is about 2°·3 in favour of the latter; but in cloudy and windy weather, the indications of both are very nearly the same.

The naked thermometer has been below 32° in every month of 1846, except June and August. In June it fell to within 1·°5, and in August to within 5° of the freezing point.

The maximum of Terrestrial Radiation in 1846, is 17·°5; in the Lake districts it amounts to 23°.

Rain.—The quantity of rain is within the 7-100th of an inch of the fall in 1845. The total quantity in 1846, is 49·134 inches, of which 17 inches fell in July and October. The quantity measured from the steeple of St James' Church, 78 feet above the street, is 35·422 inches, or 2 inches more than in the previous year. The fall at Cleator, near Whitehaven, is 55·16, or 2·16 inches more than in 1845, and 6 inches more than at this place. The fall of rain in the first six months of the year was greatly below the average; on no one day did the quantity amount to an inch in depth. In the last six months there have been three days in which the fall was between 1 and 2 inches; and two days (both in July) between 2 and 3 inches. The wet days exceed those of 1845, by 7.

Summary of Winds.—North, 33½ days; N.E., 32½ days; East, 20 days; S.E., 40¼ days; South, 73 days; S.W., 64 days; West, 58 days; N.W., 30¼ days; Calm, 13½ days. The prevailing winds in 1846 have, as usual, been South and S.W.

The following table will shew the unusual prevalence of calms and light winds in the past year:—

Year.	Calm.	Light Breeze.	Moderate Breeze.	Fresh Breeze.	Strong Breeze.	Gales.
1846 . .	90	101	80	38	28	28
1845 . .	32	77	100	40	50	66
1844 . .	49	119	103	41	24	30
1843 . .	35	79	117	54	43	37
1842 . .	26	78	149	58	31	23

In January there were 16 days of calm and light winds; in April, 23; in May, 20; in June, 16; in August, 18; in September, 22; and in December, 19 such days; the remaining days in these months principally coming under the head of "moderate breeze."

Lind's Anemometer.—The heaviest gales of wind in the past year, as indicated by this instrument, were, on January 19th, 13 lb.; February 6th, 20¾ lb.; and March 16th, 21¼ lb.—horizontal force on the square foot. The anemometer has not, however, been regularly used, and it is probable there may have been one or two gales in 1846, which have somewhat exceeded the maximum force recorded.

The following table exhibits a summary of the weather in 1846, as compared with the four preceding years:—

Year.	Clear throughout.	Cloudy without Rain.	Rain.	Sun shone out.	Snow.	Hail.	Frost.	Thunder and Lightning.
1846	27	138	200	280	7	12	30	36
1845	21	151	193	290	14	15	61	13 Th. & L. 15 L.
1844	30	164	172	292	19	12	53	15
1843	31	124	210	233	12	22	48	17
1842	43	155	167	253	6	15	25	9

In 1846, there have been seven snowy days, five of which were in November and December; twelve days of hail, one of which was in June, and two in July; thirty nights of frost, of which twenty-one were in December; and thirty-five days of thunder and lightning; and, what is most remarkable, I have only recorded one appearance of lightning, which was not followed by thunder, more or less distant.

There have been one solar, and eight lunar halos, and three appearances of the aurora borealis.

On the evening of September 30th, about 7·30^{m}·, a magnificent auroral arch was noticed here. Its breadth was about that of a rainbow, and it extended from E.N.E. to W.S.W., to the visible horizon at both extremities. Estimated altitude of centre about 70°. It remained visible nearly half an hour. From 11 P.M., to 1 A.M., there was a low arch of white light extending from N. to W., from the upper edge of which most brilliant streamers were thrown off. A dark haze, resembling smoke, mingled with the auroral light, and presented a most curious and interesting spectacle, and a form of the aurora seldom seen but in high northern latitudes. This phenomenon was seen in great splendour at Durham, and at Comrie, at Perthshire.

To sum up the peculiarities of 1846 in a few words; the summer is memorable for its excessive heat, its unprecedented mortality, and the unusual number and violence of its thunder storms:—the whole year is marked by the high temperature, dampness, and stagnant condition of the atmosphere.*

J. F. MILLER.

WHITEHAVEN, *December* 31, 1846.

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* The mean temperature of the last quarter of 1846 (Sept. 30 to Dec. 31), is 1°·115 under the average. The temperature would have been *above* the average, but for the unusual coldness of December. The mortality during the quarter is, again, very great; in this town, the deaths are 150 per cent.—and, for the whole Union, they are 130 per cent. above the average of the preceding seven years.

According to the Registrar-General's returns, the deaths throughout the kingdom, in the last quarter, are 7,311 more than the corrected quarterly average of previous years, and 13,727 more than were returned in the corresponding season of 1845.

Letter from Professor JOHNSTON, *inclosing one from* Professor MULDER *of Utrecht.*

To the Editor of the Edinburgh Philosophical Journal.

EDINBURGH, 23*d March* 1847.

MY DEAR SIR,—You will oblige me by inserting, in your ensuing Number, the enclosed translation of a letter I have received from Professor Mulder of Utrecht. The protein compounds, at the present moment, are of much interest among chemists; and it is of importance that parties who have not closely attended to the matter, should be made aware of the precise point in dispute between Mulder and Liebig, and of the exact state of our knowledge upon the subject. In this letter, Mulder has not given any of his new researches, which have already, as he informs me, made him acquainted with many new facts. The views he advances, however, are novel, and, so far, as satisfactory as they are beautiful. I am confident that, whatever the truth may prove to be in this matter, Mulder will publish it; and that no fear of personal consequences will ever cause him to deviate from that sincere path, by which alone the investigation of natural science can be successfully prosecuted.—Believe me, my dear Sir, yours very sincerely,

JAMES F. W. JOHNSTON.

UTRECHT, 12*th March* 1846.

MY DEAR FRIEND,—You will probably have read in the "Annalen der Chemie und Pharmacie" for January 1847, that Liebig now endeavours, in a covert way, to adulterate protein with sulphur under another form than that in which he originally stated it to exist. The question was first,—and this is the true state of the subject,—Can albumen, fibrin, &c., when treated with potash and acetic acid, yield a precipitate which neither discolours silver when heated upon it with potash, nor produces sulphuret of lead when heated in a solution of acetate of lead? Liebig has denied this (*Ann.*, Jan. 1846), and Laskowski likewise. According to him, the additional application of oxide of bismuth or silver was required for this purpose (*Ann.*, Mar. 1846.) I had asserted, that the latter was unnecessary, and that the reaction of the sulphur upon silver could be made to disappear by the use of potash alone.

This point demands an open publication, and not one given in an obscure and involved manner. Whether Liebig, or some one else, has been mistaken, has nothing at all to do with truth or its publication. The question is simply, What is the truth? and the answer to this is: "Albumen, fibrin, casein, &c., can be freed from sulphur by potash alone; that is, they can be deprived of that reaction which we in science distinguish as characteristic of the presence of sulphur." Sulphuret of calcium we call desulphuretted, when it yields no longer a black precipitate with acetate of lead. When it is totally converted into gypsum, we still call it *desulphuretted* (outzwaveld), although we find in it sulphuric acid.

Leibig has, in a covert manner, by a play of words, shifted and mutilated the original sulphur question. It is true, that I have stated in my last experiments, that protein, when burned with nitre, yields no sulphuric acid, or only traces of it, but the true state of the sulphur question is this: Does the reaction on silver or on acetate of lead disappear?

I must draw your attention to this, because it is of importance for the sake of science. And it is of importance for science, because its only object is truth, and not to decide, who is right or wrong.

As I am disgusted with this mutilation, I would really have declined taking up the pen to expose it, had not the observation, that protein, on being burned with nitre, produces a weighable quantity of sulphuric acid, again forced my attention upon the entire protein question.

I can mention to you, as the result of my investigations, that the sulphur exists in albumen, horse and cow hoofs, &c., in the state of sulphuret of amid $(S, N H^2)$; that protein from albumen contains variable traces of hyposulphurous acid, $S^2 O^2$, in weighable quantities; that this hyposulphurous acid is a product of the decomposition of two equivalents of sulphuret of amid $2 (S, N H^2)$, which, by the influence of alkali and two equivalents of water $(2 H O)$, produce an equivalent of hyposulphurous acid, and two equivalents of ammonia $(S^2 O^2$ and $2 N H^3)$; that, therefore, protein yields variable quantities of hyposulphurous acid, and consequently contains the sulphur under a form which has nothing in common with that in which it originally existed;—that cow-horns and fibrin consist of sulphuret of amid $(S, N H^2)$ with a body differing from protein only in the proportion of oxygen;—that protein from fibrin contains only 1 per cent. of hyposulphurous acid, being a quantity which, in the ultimate analysis, has no perceptible influence on the proportions of carbon, hydrogen, and oxygen;—that the quantity of hyposulphurous acid in protein from albumen may rise to 2·66 per cent., but is not constant; that this quantity may arbitrarily be diminished or increased, but,—and here I beg to correct a mistake, which I committed in my last experiments, and which I therefore clearly and distinctly acknowledge,—that hitherto I have been equally unable to remove from protein the last traces of hyposulphurous acid, as to separate from it the last traces of incombustible substances.

But does this imply that protein,—protein free *from sulphur*,—has no existence? On the contrary, it has now been proved to exist in the most positive manner. I hope to institute a renewed investigation of this substance, in which I take an increasing interest.

But you may ask what has become of its composition? Is it as stated by Laskowski? This has been contradicted by Fleitmann. Thus the two results were:—

	Laskowskie.	Fleitmann.	
C	54·6	54·1	53·8
H	6·0	7·1	7·3
N	23·9	15·9	16·2

I was, therefore, according to this analysis of Fleitmann, not far from the truth, when I stated the composition of protein *containing sulphur* to be:—

C	53·7	53·7
H	7·0	7·1
N	15·0	15·5

You see that Laskowski's experimental analysis was an unsuccessful one. But is Fleitmann's result correct? I will shew you, in my memoir, which I soon hope to publish, that his determination of nitrogen has been too high. Protein, containing a quantity of hyposulphurous acid, corresponding to 1·4 per cent. of sulphur, existing in that of Fleitmann, ought to yield only 14·4 per cent. of nitrogen.

Now if you reduce this protein by deducting the sulphur as hyposulphurous acid, you have:—

	By analysis.	Deducting the sulphur as hyposulphurous acid:—	
C	53·7	53·7	54·9
H	7·0	7·0	7·1
N	14·4	14·4	14·7
O	23·5	22·9	23·3
S	1·4		
	100·	98·0	100·

Again, albumen itself produced (*Phys. Chem.*, p. 326):*—

C = 76·44	C = 75·12	Deducting the sulphur as sulphuret of amid S, N H^2:—	
C (54·48)	53·6	53·6	55·1
H	7·0	6·9	7·0
N	15·7	14·5	14·9
O	22·3	22·3	23·0
S	1·4		
	100·0	97·3	100·0

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* These references are to the Dutch edition of his *Chemistry of Vegetable and Animal Physiology*; but as I have not the Dutch edition at hand, I cannot easily refer to the proper pages in the English edition.

You will see that, when we subtract from the above protein an equivalent of hyposulphurous acid, and from albumen an equivalent of sulphuret of amid, the same body is left.

I am able to adduce much more decisive instances of the reality of the existence of the compound $S\,N\,H^2$ (sulphuret of amid), in these bodies. Cow-hoofs, for instance, gave (*Phys. Chem.*, p. 564):—

	C = 76·44	C = 75·12	Deducting the sulphur, as S, N H^2 :—	
C	(51·1)	50·2	50·2	55·2
H		6·8	6·3	6·9
N		17·3	13·3	14·7
O		21·1	21·1	23·2
S		4·6		
		100·0	90·9	100·0

This, therefore, is again the same body, and it is the true composition of *desulphuretted* protein.

Cow-horns, like fibrin, yield without doubt an oxide of protein. Thus, cow-horns (*Phys. Chem.*, p. 568), gave :—

	C=76·44	C=75·12	Deducting the sulphur, as S, N H^2 :—	
C	51·0	50·1	50·1	53·8
H		6·8	6·3	6·8
N		16·2	13·2	14·2
O		23·5	23·5	25·2
S		3·4		
		100·	93·1	100·

And fibrin (*Scheik. Onderz.* iii., p. 470) gave :—

		Deducting the sulphur, as S, N H^2—	
C	52·7	52·7	53·8
H	6·9	6·8	6·9
N	15·5	14·6	14·9
O	23·9	23·9	24·4
S	1·0		
	100·	98·0	100·

In cow-horns, therefore, the same group exists as in fibrin.

I have not hitherto spoken of the phosphorus, but it is in the same state as the sulphur. I will make a more correct determination of its small proportion in fibrin, albumen, &c. But it has been found in a remarkable manner, that the phosphorus is in the same condition as the sulphur, viz. :—in the analysis of legumin. Prof.

Norton, your pupil, who is here engaged with this analysis, has, for instance, prepared legumin from green peas (which differs from that of almonds), and obtained the following result,—

		Deducting the sulphur and phosphorus as S, NH^2 and Ph, NH^2—	
C	50·8	50·8	53·6
H	6·6	6·3	6·8
N	15·6	13·9	14·7
O	23·8	23·8	25·0
S	0·8		
Ph	2·4		
	100·	94·8	100·

Legumin from peas, therefore, contains the same substance as that which exists in fibrin and horn, combined with sulphuret and phosphurut of amid. This is confirmed by all its reactions.

I will here add nothing more, but I think I have shewn, that a little more order is appearing in this chaos, and that it is far more useful to science, to distinguish between sulphur and sulphur, than to allow such a distinction to escape us.

Believe me,
Yours faithfully,
(Signed) G. T. MULDER.

To Professor JOHNSTON.

A Discourse delivered before the General Agricultural Society of Barbadoes, at its Second Half-Yearly Meeting on 22*d of December* 1846. By JOHN DAVY, M.D., F.R.S., Inspector-General of Army Hospitals, Honorary Member of the Society, &c. Published at the request of the Society, and communicated to the Editor of the New Philosophical Journal.

GENTLEMEN,—At your last, which was also your first, meeting, I had the honour of addressing you on the subject of agriculture generally, considered in some of its scientific relations. I now propose to endeavour to perform the promise I then made, and, with your leave, call your attention to the soils of Barbadoes, which, as the basis of your agriculture, cannot be too carefully studied, cannot be too thoroughly known; and such thorough knowledge, I need hardly remark, can be acquired only by minute and accurate examination.

I shall first speak of the soils of the island generally, so far as I have yet had an opportunity of observing them; secondly, of the qualities of those varieties of soils which are most strongly marked; and, lastly, I shall venture to offer a few remarks, as suggestions, on the admixture of soils and their treatment, with a view to amelioration in certain instances in which there are glaring defects.

On this occasion, as on the former, I shall have need of, and have to ask your indulgence, for the subject I am entering upon is a large and difficult one. I have not the advantage of a practical knowledge of it; the views, the information I have to offer, are more of an abstract kind than practical men commonly like, or are disposed to confide in; but being founded, as I believe they are, on scientific principles, and addressed, as I trust they are, to a scientific audience, that is, to an assemblage of well-informed men, convinced that science is essential in agriculture to promote its improvement, and give it its highest degree of perfection, I am confident I have not to ask your indulgence in vain.

Commonly, there is a certain connexion observable between the geological structure of a country and its soils. In a large number of instances this connexion is most intimate; so that the rocks constituting the hills and mountains being known, the soils of the plains and valleys may be predicated with considerable accuracy; and for this obvious reason, that the latter, by a process of decomposition and disintegration, are derived from the former. This is remarkably the case in primitive countries, in which the existing soils appear to be formed almost entirely by the processes alluded to. Occasionally, however, it is otherwise; there are countries in which there is no immediate relation, at least of dependency, between the soils at the surface and the strata on which they are incumbent, the soils not being derived from the rocky strata on which they rest. This is strongly marked in the instance of tertiary geological formations, those in the history of our globe of recent origin, formed of the detritus of older rocks, brought down by rivers or thrown up by volcanoes, deposited in the ocean; there modelled by currents, variously consolidated and altered by additions of materials which they there receive, ultimately to be raised, by forces acting from below, into the common air, to become islands or parts of continents, according to situation. Some of the most fertile parts of the earth are of this description; the majority of the West India Islands are such, and Barbadoes eminently so.

As regards the interests of agriculture, what I have just adverted to may be held to be a great advantage, as I shall, farther on, have an opportunity of pointing out; but as regards the study, the description, the comprehension of the soils of a country, it adds vastly to the difficulty, presenting, as it does, a subject of almost incalculable complication, instead of one of simplicity and uniformity.

In my preceding discourse, I took a hasty glance at the geology of Barbadoes, of its two parts so well marked by contrasts,—the smaller portion, formed of steep hills and descending valleys, consisting of beds of chalk, of various clays and of various sandstones; the larger portion of terrace-like formation, height rising above height in long continued ridges, table lands or valleys of little depression intervening, with a foundation generally of calcareous rock or marl, abounding in shells and other remains of marine animals.

In the part or district first mentioned, there is observable more relation between the soils and the rocky substrata than in the second. Nor is this surprising or inexplicable. Owing to the steepness of the hills there, their declivities are powerfully acted on by heavy rains, and there is a constant tendency to the denudation of their sides in consequence. Accordingly, in this part of the island where chalk is the substratum, a white calcareous soil, very little different from chalk, is found at the surface; where a bed of clay lies beneath, the surface soil is found to be stiff and argillaceous; where sandstone is the basis, there the soil resting on it is often little more than sand. These remarks apply chiefly to the steep declivities of the hills; less so to the valleys, especially where they open out into little plains. There, there is found a certain uniformity of soil, which, perhaps, may be considered as a mixture of all the several ingredients of the higher regions, washed down by torrents and commingled. It is a well marked alluvial soil, abounding in siliceous sand, containing more or less of clay or alumine, with a small proportion generally of magnesia and lime. It is a soil not unlike that of British Guiana, being similarly derived, but, I believe, is of a greatly better quality,—the girding mountaintains of the interior of British Guiana being chiefly of granite, and there being no back ground there of fertile chalk, as here, to furnish a supply of calcareous matter, however small.

In the other and larger portion of the island, I have said, there is less relation between the quality of the soils and the rocky beds on which they rest. In many places, even on the same estate, there may be found a considerable variety of soil and subsoil; in one spot, a calcareous marly soil; in an adjoining one, a stiff clay; and near at hand to this, a loose light soil, containing a good deal of siliceous sand, and no small proportion of calcareous earth. The principal varieties of soils over this the greater portion of the island, may be conveniently classed under a few heads, according to their composition, on which their nature and qualities depend, and in which, notwithstanding the remark I have just made as to admixture, some order of distribution is observable.

On the higher grounds a reddish-brown soil is predominant, containing a large proportion of siliceous matter in a very finely divided state, with a certain portion of clay, and an admixture in small quantities of lime and magnesia. This soil presents itself even at the edge of the cliff bordering and bounding the smaller hilly region—a situation we are sure, where it could not be brought after the ground on which it rests, coral and shell limestone, was raised from the depths of the sea, because there is no higher ground near from which it could have been conveyed by the action of water. A striking example of this is to be seen at the top of Horse Hill, immediately above the road leading down to St Joseph's Church. Of the kind of soil under consideration, excellent examples offer on the estates of Bloomsbury, Welchman's Hall, and Blackman's. These I particularise, because I have examined specimens of them, for which I am indebted to their proprietors.

Another quality of soils is that which prevails between the terrace elevations, well marked in the valley of the Sweet Bottom, and perhaps less distinctly in the more expanded and extensive valley of St George. It contains more clay than the first mentioned, a large portion of silica, and but a very small proportion of lime and magnesia. Its colour is variable ; red and brown are its predominant higher grounds.

A third variety of soil, is that which differs but little from calcareous marl ; it is incumbent on a substratum of marl, consists chiefly of carbonate of lime, contains fragments of sea shells,—contains but a small proportion of clay and silica, and less of magnesia. It is generally of a light colour. It occurs in some parts of St Philip, especially its north-eastern part, in parts of St Michael's, and also of St Lucy's, and probably elsewhere in many places.

A fourth variety is a dark soil, in some situations almost black, a colour which it owes to vegetable matter in a peculiar state of decomposition, approaching, I believe, the state of peat. This soil commonly contains a good deal of clay, with a sufficiency of calcareous matter, and of silica and magnesia. It occurs most commonly in low situations towards the sea coast, where there is little declivity, where the drainage in consequence is imperfect, and there is a tendency, when there is excess of rain, to stagnation.

I shall mention only one other variety of soil, a calcareous argillaceous marl of a grey colour, consisting of alumine, carbonate of lime, and of silica, in well adjusted proportions, with some carbonate of magnesia. The most remarkable example of it that I am acquainted with is on the Codrington College estate, below the cliff, where, thrown up into steep hills and ridges, and depressed into narrow valleys and ravines, it forms a little district apart, equally remarkable for the barrenness of its aspect, its real fertility, its abundance of water; all I believe depending on the same cause, the nature of the soil and subsoil.

Other varieties of soil might be pointed out, but, in a discourse such as this, with little advantage. They occur intermixed in one or other of the principal varieties I have enumerated, for none even of these, the principal varieties, are free from admixture to any extent. How few estates, for instance, whether situated high or low, are without marl: how few are without deposits or beds of stiff clay. I am acquainted with one estate in St Lucy's, in which there is a substratum of carbonate of lime in the form of minute ovoid granules; of another in St Michael's, in which the same substance is met with in the form of minute rhomboidal crystals uncohering, after the manner of sand; of a third in St Thomas', where adjoining a bed of calcareous marl is a deposit of siliceous earth in a very finely divided state, essentially different from, but in appearance perfectly resembling, chalk. These may be considered as curiosities; but they are not unintelligent in their indications or uninstructive. In connection with the general geological history they seem clearly to prove, as I have already stated, that the soils of Barbadoes were mainly deposited when the rocky substrata on which they rest were lying at a considerable depth in the ocean, where they were formed; and that the great variety of soils which are met with, must have been owing to the causes then in operation—rivers bringing down the detritus of the mountains of the continent, currents in the sea distributing them, and probably submarine volcanic eruptions, of which there are indications here, disturbing them, and adding new materials to complicate them the more.

These may appeaar to some, theoretical views and visionary; but they assuredly are not so. The reading of geological signs is commonly unambiguous and easy. Like the reading of a written language, it must be entered upon in its minute parts: these are, as it were its alphabet. A sea-shell, the spine of an echinus, the tooth of a shark,—the microscopic remains of infusoria, occuring mixed with water-worn particles,—whether loose, as in beds of marl, or compact, as in beds or strata of limestone, plainly declare their submarine origin. And other sources are not less clearly denoted by other appearances; the form of the minute fragments constituting volcanic dust and ashes is not to be mistaken,—each little fragment, as seen under the microscope, presenting sharp edges and acute angles; and the form of river sand, especially of hard quartz sand, altogether different from the preceding, all the edges and angles worn by attrition, is equally distinct and significant. Such geological interpretation is not merely amusing to the mind, but is valuable, I believe, in connection with agriculture, viz., by aiding to give, as it is well fitted to do, a true insight into the nature of soils.

The qualities of your soils, to which we will now proceed and give a brief consideration, as I have already observed, depend chiefly on the elements composing them. Of these qualities, generally, two views may be, and, I believe, ought to be taken; one, as regards their texture, which is commonly called their mechanical or physical condition, as to the degree of sustaining firmness,—of resistance to the implements of husbandry,—of perviousness to air and water, and of power of retaining water; the other as regards their chemical composition, considered as the source of the inorganic elements essential to the growth of plants, and which, when of the best quality, should render the soils productive without aid from manures.

Let us take now a hasty review of the principal varieties of soil before named, in relation to these qualities.

1*st, Of the Calcareous or Chalk Soil.*—This soil, besides being composed principally of carbonate of lime, in a loose finely divided state with some alumine, contains a certain portion of silica in the same state, also of carbonate of magnesia and of phosphate of lime, and doubtlessly of the vegetable alkali. From the nature of its constituent parts, and the state in which they are, this soil, physically considered, may be held to be almost the perfection of a soil; cool from its colour reflecting the sun's rays,—absorbent of moisture, and not too retentive of it, allowing the rain to descend deeply into it, and to be kept in store amongst its pores,—to ascend and be exhaled as moisture on the occasion of drought; sufficiently firm to give due support to plants, and sufficiently yielding to be easily worked, These, it must be admitted, are excellent qualities. And as regards its nourishing power, through the medium of the inorganic elements which it is capable of supplying, its character is of a very high kind, as the elements which it contains are those which appear to be most essential to the staple and more important crops of the island,—such as the sugar cane, Indian corn, yam, and others. This soil, with these excellencies, may, I believe, be held up as an *exemplar.* Its productive powers appear to be high. Without manure, I am informed, it will yield good crops, even of the sugar cane; and this successively for several years, without fresh planting; and it is worthy of remark, that the sugar, the produce of these canes, is esteemed above the average quality, and is obtained in proportionally large quantity. The estimation in which soils of this kind are held, is pretty well indicated by the rent which can be obtained for them, viz., from twenty-four to thirty dollars an acre, and even, I believe, occasionally, as high as thirty-five; and this in situations commonly of difficult access, remote from the town, and having little but the fertility of the soil to recommend them; such as the steep declivities of Mount Hillaby, the highest part of the island, the chalk soil of which, rising at least 1100 feet above the sea, is cultivated to the summit; the rugged ground in the neighbourhood of Castle Grant; and that little district of similar ground, which has received the name of Irish Town.

The other calcareous soils of the island, in widely different situations, are more or less analogous to the chalk soils, such as the marl soils already noticed, and the calcareous argillaceous soil of the Codrington College Estate; they have a great resemblance in their elementary composition, varying chiefly in the proportions of their constituent parts; and they are, I understand, all remarkably fertile.

2*dly, Of the Clay Soils.*—The qualities of these soils, as of clay soils generally, are strongly marked. It may suffice to describe them very briefly. They are retentive of moisture in a very remarkable manner, and, at the same time, little pervious to air and moisture; liable to become indurated from drought, and boggy from excess of rain, physically great defects. Their composition, I believe, is somewhat variable, always abounding in alumine, never deficient in silica; they have most commonly a small proportion of lime and magnesia, especially of the former. Those clayey soils which have a greasy feel when dry, usually contain a good deal of magnesia. In their natural state, I apprehend that the bad preponderate over the good qualities of these soil, but the reverse in their improved state.

3*dly*, *Of Sandy Soils.*—The peculiarities of these soils are the reverse of the last, being most readily pervious to air and moisture, and nowise retentive of moisture. Such is invariably their physical condition, whatever may be their chemical composition; a condition most unfavourable to fertility, and fatal to it, excepting under peculiar circumstances of situation, or seasons, such as low situations where water is abundant, or where there is a substratum or subsoil retentive of moisture, or rainy seasons. Next to these circumstances their composition is of most importance; those which are almost purely siliceous are almost barren; such exist in the parishes of St Joseph and St Andrew; whilst those which consist of comminuted shell and coral, are, in favourable circumstances, fertile.

4*thly*, *Of Alluvial Soils.*—I have already spoken of the soil which occurs in the valley of the hilly district as an example of this kind, that is, in the parishes last mentioned. The red soils of the higher grounds may also be considered as such; and even the greater part of the soils in the valley of the Sweet Bottom and of St George, and, in brief, of a large portion of the island; in some instances the tendency being to an excess of the aluminous ingredient, with undue stiffness and tenacity, in some others, and I believe more rarely, to a deficiency of it, and undue porousness. The black soils, too, may be considered as belonging to them, exclusive of the peculiarity of colour, depending, as I have already mentioned, on a peculiar state of vegetable matter. Some of these soils appear to be excellent, in which the several ingredients are in a very fine state of division, and it may be inferred, duly proportioned, as in the instance of the Bloomsbury estate soil, and some others similarly situated, and also in the instance of some of those in the lower grounds, especially in the valleys of St Andrew's and St Joseph's. Generally, I apprehend, putting aside those of the highest quality, they may be considered as average soils, and from this circumstance, that they contain commingled the essential elements of good soils; and average also, as regards their capabilities and produce.

Having thus taken a rapid glance at the qualities of some of the principal varieties of soils, I shall enter on the third part of my task, a brief consideration of the means of correcting those qualities which are most faulty. It would be tedious and out of place on this occasion, were I prepared with the requisite information, to enter into minute details on this important practical subject. It may suffice, if I venture to offer some general remarks and suggestions. As in considering the qualities of soils, so in attending to their defects, it may be well to observe the distinction between those which are physical, in contradistinction to those which are chemical. As regards the first, the main defects of faulty soils are considered in the greatest degree of generality of two different and opposite kinds, excess of tenacity or stiffness, and want of adhesion (and this in excess) or looseness; the one defect most conspicuous in the most perfect clay soils, the other in the most perfect sandy soils; the one depending on the argillaceous element being too predominant, the other, on the absence of this element in its plastic state, and on the predominence of sand, or of particles, compact and firm, without tendency to cohere, which constitute what we commonly call sand. In one or other of these two kinds of defects, I believe are comprehended all the imperfections of your soils physically viewed, and that they are to be corrected, by attending to them as principles.

If a soil is too adhesive, too stiff, presenting, during a period of drought, a hard fissured surface, during wet weather a state of bog, How is it to be treated for the removal of these great and fatal defects in the way of successful agriculture? Is it not best done by the addition of matter of a different kind, such as sand, chalk comminuted, marl, or burnt clay? Either of these will tend to diminish the stiffness, render the soil less adhesive, more pervious to air and water, especially if followed by thorough draining and subsoil ploughing, which will aid most materially in preventing the stagnation of water, and I may add, of air, in the upper stratum of soil, and conduce in a remarkable manner to preserve the soil of a just degree of humidity and of aëration, even in times of severest trial—those of flood and drought. Such measures of improvement are necessarily expensive, and demand, for their correct performance, a large outlay of capital. In England and Scotland these measures have had a most extensive trial, and, as you are no doubt aware, with most successful results, proving that capital so invested is most profitably placed, often doubling and tripling the value of the land in relation to produce and rent, and that not for a short term of years, but for an indefinite period; in brief, land so treated and improved, may be considered as reclaimed land, a great acquisition to the proprietor, a great acquisition to the country; an element I may say of wealth, and even of health; for it is clearly proved that by thorough draining, even the climate of a district may be improved, the air rendered drier, less liable to fog, even warmer in a cold country, and also free from malaria. In this island how many situations there are, and not inconsiderable tracts, which might be benefited more or less by such treatment. Wherever water rests long, after heavy rain, there we may be sure the defect in question is present, existing either in the soil or subsoil, and admits of correction.

And, I think, it cannot be doubted that improvements which have been found of such vast importance in England, so lucrative and advantageous to all concerned, would not be less so in Barbadoes. We know of one gentleman, to whose writings tropical agriculture is under no small obligations, who has commenced thorough draining on a part of his property in this island, which, from its qualities, he was of opinion required it. As the process was begun only a few months ago, and is not yet completed, it is too soon to witness any remarkable effect from it; and yet, some effect, I understand, is already apparent: a gentleman, a very competent judge, who was lately at Lamberts, has told me, that the canes over the drains were green in their stalks and growing, unaffected by the dry weather at that time prevailing, whilst those not so situated had the scorched appearance connected with the suspension of healthy active vegetation, owing to want of moisture.

If a soil is too loose (to pass to the other extreme) from excess of sand and deficiency of clay, unretentive of moisture, rain-water passing through it almost unobstructed, fatally parched in dry seasons, and never productive excepting in moist ones, what is the corrective to be applied? Is it not obviously an addition of that adhesive plastic element, clay, which is deficient? Respecting this, there can be no doubt. But a doubt may arise in the mind of the proprietor, whether the improvements calculated on, may be worth the expense that must be incurred. The determination, the solving of the doubt, must, of course, rest on a variety of circumstances, chiefly local, all connected with economical views, as the probable quantity of clay required, the distance from which it is to be brought, &c., circumstances I need not dwell on, or particularise.

Of the other order of defects, those depending upon the chemical composition of soils, I can speak with less confidence; for the subject is a difficult one, partly owing to its nature, and partly to the little attention it has yet received, especially in the West Indies. If we are to start from principles, certain questions must be asked and answered to enable us to make a successful beginning:—Such as, what is the exact composition of soil best adapted to any particular crop, for example, the sugar cane? If this can be determined, then we have a principle to guide us; we are no longer groping as it were in the dark; then the object of the planter will be to approximate as much as possible all his soils to the high standard soil; but which he can only do by knowing their composition, what elements are in excess, or in deficiency, or altogether wanting, compared with the standard. At present, I fear the state of tropical agriculture is not sufficiently advanced to enable us to say what this standard soil is, how it is constituted; and it may be long before the problem will be solved; and, certainly, never without the aid of scientific research, and the joint operation of the man of science, and of the practical agriculturist; or, what would be best, the union of the two in one, who will be capable of testing and analysing soils, with opportunities of comparing their produce with their composition, and of instituting experiments on soils artificially compounded, in further proof of the accuracy of his deductions. Such trials, allow me to observe, would be well worthy the attention of, and encouragement from this and the other Agricultural Societies of Barbadoes; and I trust there is a gentleman holding an elevated grade in one of those societies, who, as I believe, he is competent to the inquiry, will be disposed to engage in it, and will find leisure for it, during intervals that he is not performing the high function to which he has recently been called in your House of Assembly. After what I have said, I need hardly remark, that, at present, it appears to me that conjectures only can be offered on the composition of the soil best fitted for the perfection of the sugar cane. From the limited observations I have yet had an opportunity of making, I am disposed to offer it as my opinion, that soils principally calcareous, are the soils in question, such as the chalky and marly, which soils, as regards physical condition, possess all the properties most desirable in a soil; and as regards chemical composition, may be held to be nowise defective, when they contain, besides carbonate of lime and a portion of alumine, some magnesia, silica, and the phosphate of lime, and the vegetable alkali, as is commonly the case in the instances of the chalk and marl of this Island, most of them inorganic elements, which are found in the ashes of the cane, and that very generally. I have already adduced an example of what appears to be the admirable fitness of a chalky soil for cane cultivation. I did not specify the exact locality where the cane so well grows without manure, which I ought to have done, to escape perhaps, the censure of hasty generalization. The spot so pointed out to me was in a part of Welchman's Hall Estate, situated below the limitary coral cliff. Other examples might be mentioned of soils of the same kind being eminently productive. Is not the lower portion of the Codrington College estate such an instance, where the soil is an argillaceous calcareous marl, with a sufficiency of silica, and some phosphate of lime? In Antigua, there are striking examples of it: the most valuable estates of that island have a soil of calcareous marl, with a subsoil of the same, a marl similar in composition to the best in Barbadoes: And such lands, I was informed on the spot, commonly yield three hogsheads of sugar per acre, and occasionally, and not unfrequently more, and require little manure; and are as fertile now, as when they were first brought into cultivation, some of them more than a century ago. It is true, and deserving of all consideration, that there are other kinds of soils, both in Barbadoes and in Antigua, which, I am informed, yield sugar superior to the average, and in fair, although not in large proportional quantity. In these, as far as I have had an opportunity of judging, the same elements exist as in the calcareous soils, although in widely different proportions—

lime and magnesia in small quantity, and the phosphate of lime, rather in the manure than in the soil. This is a fact which suggests another train of inquiry in connection with the first, viz., what are the proportions in which certain inorganic elements, when required, should be introduced into a soil, with a view to economy and the greatest profit?

As regards the improvements of the soils of Barbadoes, whether in relation to physical condition, or chemical composition; many of the circumstances of your estates are highly favourable, and may, I think in truthfulness, be adduced as matter of congratulation. Sea-sand, composed chiefly of comminuted shells, containing besides carbonate of lime, a little phosphate of lime and animal matter, such as it is constituted on two-thirds of the shores of this island;—marl, containing the majority of the inorganic elements of the sugar cane, is of such common occurrence, that hardly an estate in the larger portion of Barbadoes is without its marl pit;—chalk, which in chemical composition is very little different from marl, and might be superior to it were it reduced to powder, in certain situations existing in vast quantities;—these are some of the happy means with which, I may say, nature has provided you, capable of effecting, I believe, most of the improvements required in the constitution of the more tenacious and too argillaceous soils, which are the most important and extensive, and with a probability, at the same time, of having their composition improved. And then for the lighter and too sandy soils, of which I believe the proportion is small, the abundance of clay at hand in moist situations affords ready and ample means of amelioration. Of the good effect of such an addition on a small scale, a striking example offers at the Crane, a spot in many respects remarkable; where we witness an apparent retiring of the sea, owing to a vast accumulation of sand on the shore, and there, in this sand, below the cliff, where once the waves broke, of which their effects are manifest in the manner the base of the cliff is worn away, there, now, fruit trees are flourishing, a garden has been formed, and all that has been found necessary in the planting of the trees, has been to introduce about their roots a little ordinary soil, the cocoa-nut, when deposited, not even requiring this. And not only do most fruit trees flourish there, such as the orange, the lime, the bread-fruit tree, &c., but even the sugar cane! I was informed by the proprietor, that it is difficult to get it to grow, but that when it passes its early stage, its growth is of extreme luxuriancy.

Such means as these I have mentioned, such facilities for the improvement of your soils, are unquestionably great advantages, and, with the qualities of your soils generally, as I have already said, a matter for congratulation; and they are advantages which will be even increased when rendered more available, as it is to be hoped they soon will be, by railway communication encircling the whole, or at least the greater part of the island,—an undertaking which, if properly conducted, cannot fail of success; and with its success, depending on judicious and intelligent exertion and enterprise, may it not be anticipated that other exertions will be made in the same spirit, conducive, directly or indirectly, to the improvement of agriculture, and to an increase of prosperity; for, allow me to ask, what branch of it, or of the manufacturing processes connected with it, whether the making of sugar or of rum, can be considered perfect, or even a near approach to perfection.

Regarding the present time, which is called, and perhaps justly, a critical time, I would observe, that *this* is remarkable in the history of mankind, to wit, that periods of difficulty have always preceded, or have always accompanied, those of improvement and advance; and, I apprehend, for the simple reason, that it is commonly only by difficulties that the mind is roused to exertion, and that no great changes and advances can be made without exertion of mind. The West Indies, I am very sanguine, with this island taking the lead, will afford another example of such success, in a flourishing and prosperous future, the result and reward of such exertion; and a bright and most encouraging example it will be, taking a view of slave and free labour, viz., that what is right morally is right—that is, advantageous, politically. And let this be proved, and I am very confident it will be proved here,—not that free labour considered by itself is cheaper than slave labour; but that the system of free labour, the system of freedom, of that system which calls into action the energies of man, is more productive, is more profitable, than the opposite degrading and torpifying system of slavery. Let this be clearly proved within the tropics, as it has already been proved so triumphantly at home, comparing the past condition of our country with its present, comparing free England with other countries of Europe hardly yet free, that have not yet *constitutions*—(a most significant term)—will not the argument be almost irresistible against slavery and slave labour? My own belief is that it will be so; and that the monstrous evils of slavery (monstrous either in act or tendency), will ere long be entirely got rid of; an aspiration, I am confident, which if not received with the same strong faith in its completion, will be met with as strong a feeling by all here assembled in favour of it, and perhaps even with a feeling stronger and more lively than I can have, many of you having witnessed the blessings of the change of the one system to the other; and are thankful for it, even with the impression of having sustained hitherto a pecuniary loss, a diminished annual income, but for which you have been more than repaid by the various benefits, moral and social, the results of emancipation.

Gentlemen, I must now hasten to conclude. There are many points of inquiry connected with your soils, that I have not even touched on, and those of no little importance,—such as the effect of burning, where there is an apparent excess of vegetable matter, as in the black soils of Barbadoes; the state and proportion of the vegetable matter in your soils generally; the question of the exhausting powers of different crops; and the state of the soil in such lands as have been under cultivation uninterruptedly for a long series of years; the effects of irrigation as a fertilizing means; the peculiar effects of different kinds of manures; besides other and hardly less interesting topics. Having occupied so much of your time, I can merely allude to these at present. Perhaps, at some other meeting, should it be your wish, I may undertake the consideration of one or more of them, provided my official duties allow me leisure, and I shall have it in my power to collect, in the interim, information such as will enable me to bring them before you with some chance of being useful, and especially in the way of opening and exciting inquiry (a word in regard to the attainment of knowledge of admirable import), without which, systematically conducted, we have no right to expect any great improvement in agriculture, whether considered as an art or a science, or, indeed, in any other departments of the arts or sciences.

Description of three varieties of Meteoric Iron.—1. *From near Carthage, Smith County, Tennessee;* 2. *From Jackson County, Tennessee;* 3. *From near Smithland, Livingston County, Kentucky.* By G. TROOST, Professor in the University of Nashville, Tennessee.

1. *Meteoric Iron from Carthage. Smith County, Tennessee.*—In vol. xlix, p. 336, of this Journal, I published a description of four varieties of meteoric iron, one of which was of the highest interest, as its fall had been witnessed by several persons. My collection has since been augmented by three other newly discovered specimens. A friend of mine, Samuel Morgan of Nashville, learned, some time in 1844, that a large mass of metal had been found in Smith County, near Carthage, Tennessee, which was considered as silver; and a small sample of it was given to him, which we both recognised immediately as meteoric iron. Mr Morgan immediately endeavoured to learn its history, and to get possession of it; but, as I observed above, it being considered a precious metal, he failed, and every thing was enveloped in mystery, till it became known that it was not silver. He learned, then, that it was in the possession of a blacksmith; that it was found about a mile from Carthage, the County seat of Smith County, and Mr M. obtained it last year for a reasonable price. It weighed 280 pounds—an oblong shapeless mass, its surface shewing here and there some projecting octahedral crystals. A piece of it was sawed off weighing 39 pounds, which now forms one of the ornaments of my cabinet. This magnificent specimen has a polished surface of

about 12 by 9½ inches. None of the metallic meteorites, that I have seen, exhibit such beautiful Widmannstattean figures which have become visible on its polished surface, without the aid of acid. It shews rhomboidal and triangular sections, which are generally a full inch, and a few more than an inch in length. These figures cover uniformly the whole of the polished surface. No heterogeneous materials are visible in it. There is only one cavity of about half an inch on its surface.

The unpolished part is partly crystallized and partly amorphous and compact. Some crystals (parts of octahedrons) project for more than an inch above the mass. The iron is very tough and malleable; and, as it contains no traces of pyrites, not susceptible of being acted upon by atmospheric agencies. A partial analysis has convinced me that it contains a notable proportion of nickel; the other components I have not ascertained.

2. *Meteoric Iron from Jackson County, Tennessee.*—I am also indebted to my friend, S. Morgan, for the knowledge of a variety of meteoric iron, which is found in Jackson County in this State. Mr M. received only a sample of it; but its history, quantity, and quality, are still kept in profound secrecy, as it is yet considered as silver, and its owner is looking out for its original deposit. The piece in my possession weighs 15 ounces. It is an accumulation of large crystals, some of an octahedral, others of a tetrahedral form—of a very soft malleable iron. Its bold and vivid crystals distinguish it from the other Tennessee meteoric iron.

It was accompanied by some fragments of the crust of meteoric iron, weighing 3½ ounces. It is a hydroxide of iron, of a brown and yellow colour, penetrated here and there with metallic iron itself, differs very much from the last-named iron. This crust, and the bold crystalline structure, shews that the original mass must have been large.

3. *Meteoric Iron from Livingston County, Kentucky.*—Some six or seven years since a piece of metal was handed me with the request that I would see how much silver it contained. When I told the person who shewed it, that it did not contain silver, and was only iron, he became displeased, and departed without answering my queries as to its locality, quantity, &c. Some years after I received another piece of it from a different person. I convinced him that it was iron; but all the information I could obtain was that an abundance of it was found, and as he intended to purchase the land on which it occurred, he refused to mention the locality, but promised to send me a large piece of it. The man did not keep his promise, and I have not heard of him since. But some time last year Colonel Player of Nashville mentioned to me that he had the offer of a tract of land on which such iron-ore (shewing the identical meteoric iron) was in abundance; he thought it was ore of an excellent quality, and that it did not require any preparation, and

would be worked in the same state it was found in, shewing me a cold chisel made of it. I told him to examine the land and see whether an abundance of it really did exist, mentioning, at the same time, *that* I was convinced that but a single, or at least very few pieces of it could be found, because it was meteoric iron, which, so far as it is known, falls only in single masses. Colonel Player went to the spot and then learned that only a single piece of it had been found. From this gentleman I learned that the original piece was pretty large, but that it had been cut up and worked in the blacksmith's shop; that the only piece now existing in its natural state, and which he had in his possession, was of about 8 to 10 pounds; part of it, together with the cold chisel mentioned above, is now in my possession.

This piece weighs 4¾ pounds. It is a remarkable variety, has a fine granular fracture, similar to that of steel, is very compact, and has no traces of crystals, or of a crystalline structure. It is a shapeless mass and has a rough surface, where it has not been cut—it has the properties of steel; in fact the above-mentioned chisel is equal to one made of cast-steel. An incomplete analysis has given me, 10 per cent. of metal mostly nickel. It was found near Smithland, Livingston County, Kentucky.—*American Journal of Science and Arts,* Vol. ii. p. 356.

SCIENTIFIC INTELLIGENCE.

METEOROLOGY.

1. *Effects of Diminished Atmospheric Pressure on Animals.*—In the heights of the Cordillera the effect of the diminished atmospheric pressure on the human frame shews itself in intolerable symptoms of weariness and an extreme difficulty of breathing. The natives call this malady the *Puna* or the *Soroche*; and the Spanish Creoles give it the names of *Mareo* or *Veta.* Ignorant of its real causes they ascribe it to the exhalations of metals, especially antimony, which is extensively used in the mining operations. The first symptoms of the veta are usually felt at the elevation of 12,600 feet above the sea. These symptoms are vertigo, dimness of sight, dulness of hearing, pains in the head, and nausea. Blood flows from the eyes, nose, and lips. Fainting fits, spitting of blood, and other dangerous symptoms, usually attend severe attacks of veta. The sensations which accompany this malady somewhat resemble those of sea-sickness, and hence its Spanish name *Mareo.* But sea-sickness is unaccompanied by the distressing difficulty of breathing experienced in the veta. This disorder sometimes proves fatal, and I once witnessed a case in which

death was the result. Inhabitants of the coast and Europeans, who for the first time visit the lofty regions of the Cordillera, are usually attacked with this disorder. Persons in good health and of a spare habit usually recover from it, but on plethoric and stout individuals its effects are frequently very severe. After an abode of some time in the mountainous regions the constitution becomes inured to the rarefied atmosphere. I suffered only two attacks of the veta; but they were very severe. The first was on one of the level heights; and the second on the mountain of Antaichahua. The first time I ascended the Cordillera I did not experience the slightest illness, and I congratulated myself on having escaped the veta; but a year afterwards I had an attack of it, though only of a few hours duration. The veta is felt with great severity in some districts of the Cordillera, whilst in others, where the altitude is greater, the disorder is scarcely perceptible. Thus it would seem that the malady is not caused by diminished atmospheric pressure, but is dependent on some unknown climatic circumstances. The districts in which the veta prevails with greatest intensity are, for the most part, rich in the production of metals, a circumstance which has given rise to the idea that it is caused by metallic exhalations.

I have already described the effect of the Puna climate on beasts of burden. Its influence on some of the domestic animals is no less severe than on the human race. To cats it is very fatal, and at the elevation of 13,000 feet above the sea those animals cannot live. Numerous trials have been made to rear them in the villages of the upper mountains, but without effect; for, after a few days' abode in those regions, the animals die in frightful convulsions; but when in this state they do not attempt to bite. I had two good opportunities of observing the disease at Yauli. Cats attacked in this way are called by the natives azorochados, and antimony is alleged to be the cause of the distemper. Dogs are also liable to it, but it visits them less severely than cats, and with care they may be recovered. —(*Travels in Peru, by J. J. Von Tschudi*, p. 296.)

2. *Presence of Sulphur in Metallic Substances struck by Lightning.*—On Sunday, the 14th June 1846, the parish church of Saint Thibaud-de-Couz, three leagues from Chamberry, was struck by lightning; the church was filled with dense smoke, accompanied by a strong smell resembling that of gunpowder. The gilt frame of a large picture was almost entirely blackened, and six gilt chandeliers were all rendered as black as copper would be after long exposure to sulphuretted hydrogen.

M. Bonjean procured some powder by scraping the surface of the chandeliers, which had been most strongly coloured; by treating it with aqua regia, he obtained a solution, in which solution of nitrate of barytes gave a white precipitate insoluble in nitric acid.—(*Journ. de Pharm. et de Ch., Decembre* 1846.)

3. *Note on the Atmospheric Dust which fell at Genoa on 16th of May*, 1846. (Extract of a Letter from M. Ehrenberg to M. F. J. Pictet.)—We have given, in the 5th Number of the *Archives* (vol. ii. p. 87) some details respecting an atmospheric dust which fell at Genoa after a storm, on the 16th May 1846. I perceived in it many carapaces of infusoria, and I therefore hastened to send a specimen to M. Ehrenberg, as the most competent observer of all that relates to the history of microscopic animals. This expert micographer instantly examined it, and transmitted to me the following details resulting from his investigation. F. J. P.

" Immediately after receiving the atmospheric dust, or powder of Genoa, which you sent me, I examined it, and have found it very interesting. My observations have not yet been very numerous, but they have been made with care, and have enabled me to recognise in the dust 46 species of minute organisms. It is ferruginous, as the colour indicates ; the iron probably arises from the gallionelles, which are very numerous in it. The form of this dust, its colour and composition, render it in every respect identical with that which fell in the Atlantic Ocean, near Cape de Verd Islands, and with that which was collected at Malta in 1834. But it is not only the colour, form, and composition in general which are identical in the dusts collected in these different times and places ; the same species which predominate in the one, likewise predominate in the other. It appears natural to suppose that these sands come from Africa ; however, they contain not only continental forms, but also very distinct marine organisms. The Diploneis, for example, are abundant in all these dusts, and in all seas, but have not been hitherto found in any fresh water, properly so called. The dust of Genoa, like the rest, contains many forms special to South America, and wants, on the contrary, such as are peculiar to Africa.

" It appears to me always more and more evident that the dust of the Sirocco is a phenomenon more extensive and general than is supposed, and deserves to be studied with great care. Does it come from America ? Has it its origin in Africa ? These are questions which can be answered only after numerous observations.

" Such, Sir, are the facts which I can at present communicate to you respecting the dust you have sent me. By continuing my researches, it is probable that I shall see the number of organic forms increase still more. I wished to delay no longer in assuring you that the present subject is one of great interest, and that it is very important that the phenomena connected with it should be carefully observed."

Similar dust has been collected at Malta, Cape de Verd Islands, and in the Atlantic Ocean, as far as 800 sea miles from the west coast of Africa. I have seen specimens collected in the years 1830, 1834, 1836, 1838, 1846. They are all so identical, that one would believe they had been taken from the same packet.

The following is a catalogue of the species observed in the dust of Gènoa :—

A. Polygastrica.

- Campylodiscus clypeus.
- Chætoglena volvocina.
- Cocconeis lineata.
- Diploneis didyma.
- Discoplea atmospherica.
  - al. sp.
- Eunotia amphioxys.
  - diodon ?
  - gibberula.
  - monodon.
  - tridentula.
- Fragilaria ?
- Gallionella crenata.
  - distans.
  - granulata.
  - procera.
- Navicula.
- Pinnularia borealis.
- Stauroneis.
- Surirella croticula.
- Synedra entomon.
  - ulna.

B. Phytholitharia.

- Amphidiscus anceps.
  - clavatus.
  - Martii.
- Lithasteriscus tuberculatus.
- Lithodontium hursa.
  - falcatum.
  - furcatum.
  - nasutum.
  - platyodon.
  - rostratum.
- Lithostylidium amphiodon.[*]
  - clava.
  - formica.
  - quadratum.
  - rude.
  - serra.
  - spiriferrum.

ASTRONOMY.

4. *Calorific Power of the Light of the Moon.* (*A Letter from M. Meloni to M. Arago, and inserted in the Chemist, May* 1846.)—M. Melloni has shewn, beyond a doubt, that the rays of the moon are calorific to a slight extent. It was done by concentrating the rays of the moon with a lens over three feet in diameter, upon his thermoscopic pile. The needle was found to deviate from 0°·6 to 4°·8, according to the phase of the moon. Numerous precautions had to be attended to, so as to avoid all error arising out of currents of air, &c. (J. L. S.—*American Journal of Science and Arts*, vol. ii., No. 5, p. 256.)

5. *The Central Sun.*—At the close of the meeting (on the 14th of December) of the Royal Irish Academy, Sir William Hamilton announced that he had just received from Professor Mædler, of Dorpat, the extraordinary and exciting intelligence of the presumed discovery of a central sun. Professor Mædler's essay on the subject (*Die Central Sonne*, Dorpat, 1846) was also exhibited by Sir William Hamilton on the same evening to several members of the Academy ; but, as this work may not for some time to come be generally known, or indeed, easily procurable in this country, we

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[*] Biblioth. Univ. de Genéve, Supplement, No. 6, p. 203.

believe that our readers will thank us for publishing a sketch of the results which it contains, and which were briefly stated on the evening already mentioned. This course will also serve to correct any misconception on the subject, which the unexpected nature of the communication, and the consequent absence of many of the members of the Academy, may possibly have occasioned, or at least allowed to arise. By an extensive and laborious comparison of the quantities and directions of the proper motions of the stars in various parts of the heavens, combined with indications afforded by the parallaxes hitherto determined, and with the theory of universal gravitation, Professor Mædler has arrived at the conclusion that the Pleiades form the central group of our whole astral or sidereal system, including the Milky Way and all the brighter stars, but exclusive of the more distant nebulæ, and of the stars of which those nebulæ may be composed; and within the central group itself he has been led to fix on the star Alcyone (otherwise known by the name of η Tauria), as occupying exactly or nearly the position of the centre of gravity, and as entitled to be called the central sun. Assuming Bessel's parallax of the star 61 Cygni, long since remarkable for its large proper motion, to be correctly determined, Mædler proceeds to form a first approximate estimate of the distance of this central body from the planetary or solar system; and arrives at the (provisional) conclusion, that Alcyone is about 34,000,000 times as far removed from us, or from our own sun, as the latter luminary is from us. It would therefore, according to this estimation, be at least a million times as distant as the new planet, of which the theoretical or deductive discovery has been so great and beautiful a triumph of modern astronomy, and so striking a confirmation of the law of Newton. The same approximate determination of distance conducts to the result that the light of the central sun occupies more than five centuries in travelling thence to us. The enormous orbit which our own sun, with the earth and the other planets, is thus inferred to be describing about that distant centre, not indeed under its influence alone, but by the combined attraction of all the stars which are nearer to it than we are, and which are estimated to amount to more than 117,000,000 of masses, each equal to the total mass of our own solar system, is supposed to require upwards of 18,000,000 of years for its complete description, at the rate of about eight geographical miles in every second of time. The plane of this vast orbit of the sun, is judged to have an inclination of about 84 degrees to the ecliptic, or to the place of the annual orbit of the earth; and the longitude of the ascending node of the former orbit in the latter is concluded to be nearly 137 degrees. The general conclusions of Mædler respecting the constitution of the whole system of the fixed stars, exclusive of the distant nebulæ, are the following:—He believes that the middle is indicated by a very rich group (the Pleiades), containing many considerable individual bodies, though at immense distances from us. Round this he supposes there is a zone, proportionably poor in stars, and then a broad rich ring-formed layer, followed by an interval comparatively devoid of stars, and afterwards by another annular and starry space, perhaps with several alternations of the same kind, the two outmost rings composing the two parts of the Milky Way, which are confounded with each other by perspective in the portions most distant from ourselves. Professor Mædler has acknowledged in his work his obligations, which are those of all inquirers in sidereal astronomy, to the researches of the two Herschels, Sir William and Sir John. The views of Sir William Herschel respecting the relation of our solar system to the Milky Way will naturally recur to the recollection of our readers; and while astronomers are anxiously awaiting the shortly expected appearance of the complete account of Sir John Herschel's observations on the southern nebulæ, the following passage of a letter, which was written in 1835 by that illustrious son of an illustrious sire, from the Cape of Good Hope to Sir William Hamilton, may be read with peculiar interest, from the agreement between the views it expresses, and some of those to which Professor Mædler had been led. In the letter just referred to (from which an extract was published at the time) Sir John Herschel expressed himself as follows:—"The general aspect of the southern circumpolar region, including in that expression 60° or 70° of S. P. D., is in a high degree rich and magnificent, owing to the superior brilliancy and larger development of the Milky Way; which, from the constellation of Orion to that of Antinous, is one blaze of light, strangely interrupted, however, with vacant and almost starless patches, especially in Scorpio, near α Centauri and the Cross; while to the north it fades away pale and dim, and is in comparison hardly traceable. I think it is impossible to view this splendid zone, with the astonishing rich and evenly distributed fringe of stars of the third and fourth magnitudes, which form a broad skirt to its southern border, like a vast curtain, without an impression, amounting to a conviction, that the Milky Way is not a mere stratum, but an annulus; or at least, that our system is placed within one of the poorer and almost vacant parts of the general mass, and that eccentrically, so as to be much nearer to the parts about the Cross than to that diametrically opposed to it.—*Vide* an article on Mædler's Views, by Professor Alfred Gautier of Geneva, in the 9th Number of the Supplement of the *Bibliotheque Universelle*, 24th October 1846.

MINERALOGY.

6. *Cryptolite*; *By F. Wöhler* (*Pogg. Ann., No.* 3, 1846; *Phil. Mag.* xxix. 31*st July* 1846.)—Cryptolite is a phosphate of the oxide of cerium, found in the sea green or reddish apatite of Arendal in Norway. It becomes apparent when the apatite is placed in

large pieces in dilute nitric acid, appearing, as the apatite dissolves, in the form of fine crystalline needles of about a line in length. It occurred only in the reddish variety of apatite, and constituted but 2 or 3 per cent. of the mass. It crystallizes in transparent apparently six-sided prisms of a very pale wine colour; specific gravity 4·6. It undergoes no change at a moderate heat. It afforded, on analysis, peroxide of cerium 73·70, protoxide of iron 1·51, phosphoric acid 27·37 = 102·58. The excess arises from the cerium having been determined as peroxide instead of protoxide, in which latter form it evidently exists in the mineral. The presence or absence of didymium and lanthanum remains undetermined.

It is probable that the apatate of Arendal contains another cerium mineral which is soluble, and may possibly be monazite, Silliman.

7. *Amalgamation of Wrought Iron, Cast Iron, and Steel, so as to prepare them for Fine Gilding. By R. Boettger.* Poggend. Ann. 1846.—Place in a glazed earthenware or porcelain vessel, 12 parts, by weight, of mercury, 1 of zinc, 2 of sulphate of iron, 12 of water, and 1½ of hydrochloric acid of 1·2 sp. grav., then introduce the iron or steel into the mixture, which is to be heated to ebullition. In a little time the objects become covered with a thin coating of mercury, which enables us to apply immediately the amalgam of gold that is used in the gilding. All that is now necessary is to apply a strong heat, which will drive off the mercury, and the trace of zinc that may have attached itself to the iron, leaving a surface of pure gold. By the ordinary way, it becomes necessary to cover the iron first with a coat of copper.

8. *Xylite, a new Mineral. By M. Hermann* (*Journ. d'Erdmann*, xxxiv. 177).—Xylite is from the copper mines of the Ural. It is essentially a silicate of iron; it has a deep brown colour, a fibrous fracture, specific gravity 2·935; hardness inferior to calcspar. It is but slightly attacked by acids. It contains silica 44·06, peroxide of iron 37·84, lime 6·58, magnesia 5·42, oxide of copper 1·36, water 4·70 = 99·96.

9. *Antimoniate of Lead. By M. Hermann,* (*Journ. d'Erdmann*, xxxiv. 177).—This ore is from the district of Nertschinsk in Russia. It is amorphous, with a compact texture and resinous lustre; colour generally sulphur-yellow, sometimes greyish, green, or black; specific gravity 4·60—4·76. It consists of antimonic acid 31·71, oxide of lead 61·83, water 6·4 + 6.

10. *Loxoclase. By A. Brethaupt,* (*Phil Mag.*, August 1846.)—Loxoclase is near feldspar in its characters, and comes from Hammond, in the state of New York. It is distinguished by having an oblique cleavage, parallel with the long diagonal, though not always very distinct, and from this its name is derived. Hardness a little above ordinary feldspar; specific gravity, 2·609—2·620; colour, yellowish-grey, whitish and bluish-grey. It has the general crystal-

line form of common feld-spar, with the angle M. T.=120° 15. It fuses before the blowpipe with difficulty, and shews in the outer flame an intense soda reaction. Composition, according to M. Plattner, silica 63·50, alumina 20·29, oxide of iron 0·67, potash 3·03, soda 8·76, lime 3·22, water and fluoride of silicon 1·23. It occurs with pyroxene, graphite. and calc-spar.

11. *Native Tin.* (*Jour of Prackt. Chem.* xxxiii., 300).—According to Hermann, native tin occurs in the gold washings of the Ural, in small grey metallic grains, containing also some lead.

12. *Aragonite and Calc-spar.* (*L'Institut*, No. 654, July 15, 1846, p. 240).—MM. Silbermann and P. A. Favre have arrived at the conclusion, that these two minerals are isomeric, and that the dimorphism of carbonate of lime is due to this principle. The arragonite is shewn to be the higher of the isomeric compounds.

13. *On Iolite.*—In an elaborate memoir on the mineral Iolite, including its several varieties, M. Haidinger shews that the so-called mineral species, Pinite, Fahlunite, Weissite, Bronsdorffite or Hydrous Iolite, Gigantolite, Chlorophyllite, Praseolite, Esmarkite, and perhaps also Oosite, are only metaphors of Iolite, arising from a change of composition in crystals of this species.* (*American Journal of Science and Arts*, Vol. ii., No. 6, 2d series, p. 513.) Also (*Pogg. Annal.*, 1846, lxvii., 441).

GEOLOGY.

14. *Cocoa Trees in Stratified Sand.*—It was one o'clock, P.M., when we reached a little village called Pebla, which is situated on the coast, about five leagues to the north of the mouth of the Rio San Francisco. This was the termination of my sea voyage, as the heavy surf which breaks over the shallow bar of that river will not allow jangadas to enter it. The village is situated a little way inland, and is hidden from the sea by a high embankment of sand, which at this place is very much drifted by the wind; it is, however, recognised at a considerable distance, from the number of tall cocoa-nut trees which grow near the shore. I was here particularly struck with a fact, which goes a great way to explain the phenomenon of a stem of a fossil tree being found passing through several strata of sandstone. Many of the cocoa-nut trees have their stems embedded, to the depth of 50 feet and upwards, in the embankment of sand which stretches along the shore, and in many places is several hundred feet broad; some of them, indeed, are so deeply embedded, that the nuts can be gathered without climbing the tree. Now as this sand has accumulated at different periods, particularly during the prevalence of the north-east trade-wind, it must present, if ever it becomes hardened, a vast number of irregularly horizon-

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* Mr Haidinger cites the suggestion made by Mr Dana, in his Mineralogy (2d Edit. p. 307), that several of the above enumerated minerals were derived from the alteration of Iolite.

tal beds, through which the stems of the palms will be found to pass. —(*Travels in the Interior of Brazil, by George Gardner*, p. 110.)

15. *Eruption of Mount Hecla.*—The Royal Academy of Sciences at Göttingen petitioned the King of Hanover to send commissioners into Iceland, at the government expense, for the purpose of observing this phenomenon; and two distinguished geologists, Messrs Berez and Sartorius, were nominated for that duty.

16. *Observations on the petrifaction of Shells in the Mediterranean. By MM. Marcel de Serres and L. Figuier.*—The object of the authors of this memoir is to prove that phenomena are now taking place in our own seas, altogether similar to those which have produced the petrifaction of shells. They shew that, in the double relation of chemical composition and mode of petrifaction, the shells preserved in the bottom of the sea, are in every respect like those which belong to the tertiary formations. The difference which exists, between the mode of substitution at present times, and that of geological times, consists in this, that the petrifaction found in the historical epoch, usually present a more crystalline texture. They do not, however, become very compact,. till they have passed through different degrees. The investigations of MM. Marcel de Serres and Figuier have related chiefly to oysters, pectens, venus, petunculi, and cardium, genera found, at the same time, preserved in tertiary formations, and in recent banks of rivers. (*Supplement a la Biblioth. Univ. de Geneve*, No. 8, p. 421.

17. *On Slaty Cleavage.*—One law respecting slaty cleavage was announced in 1831, by Professor Sedgwick,* and is now well known: that law is, that the cleavage planes maintain their parallelism over extensive areas irrespective of the varying position of the beds which they cut through, or of the mineral character of the beds. Another law respecting slaty cleavage was detected by the author† in the progress of his tour, and is the following: viz. that the strike of the cleavage coincides with the strike of the bedding, whenever the latter continues uninterruptedly the same for a considerable distance; but when the strike of the bed is inconstant, and shifts at short intervals, then the cleavage planes hold their course right on, irrespective of the varying position of the planes of bedding; in other words, that the strike of the cleavage coincides with the prevailing strike of the beds in each district, and does not vary with the subordinate and local irregularities in the strike of the beds. Whence it follows, that

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* Geol. Trans., 2d series, vol. iii., p. 68.

† While the author was drawing this conclusion from his observations in Wales a nearly similar law was announced to the British Association at Cork, by Professor Phillips, in the following terms:—"The cleavage planes of the slate rocks of North Wales are always parallel to the main direction of the great anticlinal axis, but are not affected by the small undulations or contortions of these lines. In North Wales they maintain the same direction for 50 miles, not varying more than 2 or 3 degrees." (See Athenæum, 2d September 1843.)

the strike of the cleavage in a district is more constant and regular than the strike of the beds.—(*Quarterly Journal of the Geological Society*, No. 7, p. 309.)

BOTANY.

18. *On the Production of Vanilla in Europe.*—In marshy bushy places on this journey, I saw many plants of the *Vanilla planifolia*, seldom bearing flowers, and more rarely producing fruit. It has now been satisfactorily determined, that this is the species from which the true vanilla of commerce is procured. In Mexico it is extensively cultivated for the sake of its fruit, which it yields abundantly: while the plants which have been introduced into the East Indies, and the hothouses of Europe, though they have frequently produced flowers, have very seldom perfected their fruit. Dr Morren of Liége was the first to study attentively the natural history of this plant, and to prove experimentally that the fruit of the Vanilla may be as freely produced in our hothouses as it is in Mexico. He has discovered that from some peculiarities in the reproductive organs of this plant, artificial fecundation is required. In the year 1836, a plant in one of the hothouses in the botanic garden at Liége produced fifty-four flowers, which having been artificially fecundated, exhibited the same number of pods, quite equal to those imported from Mexico; and, in 1837, a fresh crop of about a hundred pods was obtained upon another plant by the same method. He attributes the fecundation of the plant in Mexico to the action of some insect which frequents the flower; and hence accounts for the non-production of fruit in those plants which have been removed to other countries. There can be no doubt that this plant is as perfectly indigenous to Brazil as it is to Mexico; but it is no less certain that its fruit is there seldom matured. Is this also to be attributed to the absence of the means by which nature is supposed to effect fecundation in Mexico? This is a subject which, as Professor Morren justly observes, well deserves attention in a commercial point of view, since his experiments go to prove that in all intertropical countries vanilla might be cultivated, and a great abundance of fruit obtained.*—(*Travels in the Interior of Brazil. By George Gardner*, p. 296.)

19. *Phosphorescent Fungus.*—One dark night, about the beginning of December, while passing along the streets of the Villa de Natividade, I observed some boys amusing themselves with some luminous object, which I at first supposed to be a kind of large firefly; but on making enquiry, I found it to be a beautiful phosphores-

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* See Professor Morren's paper " On the Production of Vanilla in Europe," in Taylor's Annals of Natural History, vol. iii., p. 1.

cent fungus, belonging to the genus *Agaricus*, and was told that it grew abundantly in the neighbourhood, on the decaying leaves of a dwarf palm. Next day I obtained a great many specimens, and found them to vary from 1 to 2½ inches across. The whole plant gives out at night a bright phosphorescent light, of a pale greenish hue, similar to that emitted by the larger fire-flies, or by those curious soft-bodied marine animals, the *Pyrosomæ.* From this circumstance, and from growing on a palm, it is called by the inhabitants "Flor de Coco." The light given out by a few of these fungi, in a dark room, was sufficient to read by. It proved to be quite a new species; and since my return from Brazil, has been described by the Rev. Mr Berkeley, under the name of *Agaricus Gardneri*, from preserved specimens which I have brought home. I had already named it *A. phosphorescens*, not being aware at the time I discovered it that any other species of the same genus exhibited a similar phenomenon; such, however, is the case in the *Agaricus Olearius* of De Candolle; and Mr Drummond, of the Swan River Colony, in Australia, has given an account of a very large phosphorescent species, occasionally found there.* — (*Gardner's Travels in the Interior of Brazil*, p. 346.)

20. *The Chirimoya* (*Anona tripetala*), *the most delicious of fruits.*—Of tropical fruits, the number is not so great in Peru as in the more northerly district of Guayaquil. But there are some Peruvian fruits, the delicious flavour of which cannot be excelled. One of these is the Chirimoya (*Anona tripetala*). Hanke, in one of his letters calls it "a master-work of nature." It would certainly be difficult to name any fruit possessing a more exquisite flavour.

In Lima the chirimoya is comparatively small, often only the size of an orange. Those who have tasted it only in Lima, can form but a very imperfect idea of its excellence. In Huanuco, its indigenous soil, it grows in the greatest perfection, and often attains the weight of sixteen pounds and upwards. The fruit is of roundish form, sometimes pyramidal or heart-shaped, the broad base uniting with the stem. Externally it is green, covered with small knobs and scales, and often has black markings like net-work spread over it. When the fruit is very ripe, it has black spots. The skin is rather thick and tough. Internally the fruit is snow-white and juicy, and provided with a number of small seeds, well covered with a delicate substance. The chirimoyas of Huanuco are also distinguished from those of the coast by having only from four to six seeds; whereas, on the coast they are found with from twenty-five to thirty. The question as to what the taste of this fruit may be compared with, I can only answer by saying, that it is incomparable. Both the fruit and flowers of the chirimoya emit a fine fragrance, which, when the tree is covered with blossoms, is so strong as to be almost overpowering. The tree which bears this finest of all fruits is from fifteen to twenty feet high. It has a broad flat top, and is of a pale green colour.—(*Travels in Peru, by J. J. Von Tschudi*, p. 185.)

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* Hooker, Journal of Bot., vol. i., p. 215.

21. *Effects of Datura sanguinea, R. Pav.*—To this plant the natives give the names *Huacacachu*, *Yerba de Huaca*, or *Bobachevo;* and they prepare from its fruit a very powerful narcotic drink, called *tonga*. The Indians believe that by drinking the tonga they are brought into communication with the spirits of their forefathers. I once had an opportunity of observing an Indian under the influence of this drink. Shortly after having swallowed the beverage he fell into a heavy stupor: he sat with his eyes vacantly fixed on the ground, his mouth convulsively closed, and his nostrils dilated. In the course of about a quarter of an hour his eyes began to roll, foam issued from his half-opened lips, and his whole body was agitated by frightful convulsions. These violent symptoms having subsided, a profund sleep of several hours succeeded. In the evening I again saw this Indian. He was relating to a circle of attentive listeners the particulars of his vision, during which he alleged he had held communication with the spirits of his forefathers. He appeared very weak and exhausted.

In former times the Indian sorcerers, when they pretended to transport themselves into the presence of their deities, drank the juice of the thorn-apple, in order to work themselves into a state of ecstasy. Though the establishment of Christianity has weaned the Indians from their idolatry, yet it has not banished their old superstitions. They still believe that they can hold communication with the spirits of their ancestors, and that they can obtain from them a clue to the treasures concealed in the *huacas*, or graves; hence the Indian name of the thorn-apple—*Huacacachu*, or grave-plant. (*Travels in Peru, by Dr J. J. Von Tschudi*, p. 269.)

ZOOLOGY.

22. *On the Flight of the Flying Fish.*—In those latitudes, it was also curious to watch the flight of the flying-fishes (*Exocetus volitans*), whole shoals of which rose quite close to the ship; and I have perfectly satisfied myself, not only on this occasion, but during the several times I have crossed the ocean, that they make use of their pectoral fins as wings, during the time they remain above water. This fact I was particularly desirous to ascertain, as Cuvier, and all other authors I have consulted on the subject, except Humboldt, deny that this is the case.* The distance to which they fly is sometimes

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* The following is the statement of Baron Cuvier:—"Leur vol n'est jamais bien long; s'elevant pour fuir les poissons voraces, ils retombent bientôt, parce que leurs ailes ne leur servent que de parachutes."—*Le Règne Animal*, tome ii., p. 287, Edit. 1829.

very short; at others I have watched them skimming along till the eye almost lost sight of them. I should say that they frequently extend their flight to 300 yards. The height to which they rise above the surface of the sea does not usually exceed 3 or 4 feet; but that they rise higher is well known, from the fact that they not unfrequently fly on board ships, which are from 10 to 15 feet out of the water. When the sea is calm, they shoot along on the same plane, like an arrow, and the impulse they acquire on leaving the water, appears to be that alone which impels them onward. The first time I discovered that they certainly use their fins as wings, was one day when a rather high swell was running; a good many fish were rising, but not in great numbers at a time. Solitary individuals could be followed by the eye to a great distance, but during their progress they did not keep on the same plane, nor did the course of their flight form the segment of a circle, but they could most distinctly be seen rising and falling over the heavy swell, keeping always at about the same height above the water, just as a bird would do, the albatross for example, when skimming along in search of food. The only time I ever saw distinctly the fins moved in the manner of wings, was in the South Atlantic Ocean; one beautifully clear day, when we were running quietly along under the influence of a light breeze, several large dolphins were playing about, one of which we saw give chase to a flying fish; the latter rose, but its flight was followed by the dolphin. It fell close to the ship, and in attempting to rise again, the impulse was not sufficient to throw it completely out of the water; it flew along with its tail nearly out of the sea, for about a yard, when it fell a prey to its pursuer. Several of the other passengers were watching it also, and by all of us the large fins were seen to be worked with great rapidity. I agree with Humboldt,* that these fishes do not always rise out of the water to escape from their enemies, as they often spring up close to ships when there are no signs of large fishes being near. Why should the flying fish, having the power to do so, not enjoy a flight in the air, quite as much as the duck does a dive under the water, or land animals the luxury of bathing?—(*Gardner's Travels in the interior of Brazil*, p. 555.)

23. *Osteography, or Iconographic description of the Skeleton and Dental System of the Five Classes of Vertebrate Animals. By M. Ducrotay de Blainville, 20th fasciculus; Genus Rhinoceros.*—M. de Blainville has published the twentieth part of his great work on the osteography of the vertebratæ. This number contains the history of rhinoceroses, which had great need of being elucidated by a judicious critic, and a comparative observation of the numerous materials which exist in the different European collections. The rhinoceroses,

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* Personal Narrative, vol. ii., p. 15.

unknown to the Greeks previous to the Roman conquests, exist in our day only in the warmer regions of the old continent; but they have been found in a fossil state in a multitude of European localities, and their remains have been described by numerous authors. These animals form so distinct a genus, that we rarely find errors in the generic determination of their fossil bones; but the case is not the same with regard to the discrimination of species. If we adopt as real all that have been indicated, we must believe that, during the conclusion of the tertiary epoch and the commencement of the diluvian epoch, at least eighteen species of rhinoceros inhabited Europe! Extensive researches continued during three years, and the comparison of a great quantity of bones, have convinced M. Blainville that the number must be greatly restricted, and that the greater part of these species had been established without sufficient grounds. We shall not attempt to give in this place an extract of the numerous anatomical and odontographic details contained in this voluminous memoir; we shall merely point out the principal conclusion to which the learned author has come to with regard to the number of species, and their geographical and geological distribution.

M. de Blainville admits five living species as indisputable: two of which are African: the rhinoceros of the Cape (*Rh. bicornis*), and the camus rhinoceros of Southern Africa (*Rh. simus*); three are Asiatic; the rhinoceros of India (*Rh. unicornis*), the rhinoceros of Java, with one horn and $\frac{2}{2}$ incisors (*Rh. Javanus*), and that of Sumatra, with two horns and probably $\frac{2}{1}$ incisors (*Rh. Sumatranus*). Some authors distinguish two other African species, which M de Blainville considers imperfectly characterised. Some accounts would likewise lead us to believe in the existence of a rhinoceros in Africa with one horn, which would form another species to be added to the preceding.

Among the fossil rhinoceroses, M. de Blainville admits but three European species as certain. The first is the rhinoceros with partitioned nostrils (*Rh. tichorhinus*). This species, destitute of incisors, had three toes on each foot, the cranium elongated, the nostrils separated by a bony partition; its nose was provided with two horns; its molars approached those of the Rhinoceros camus, its bones were short and strong, and its body covered with hair. M. de Blainville remarks on this subject, that these hairs have sometimes been erroneously described as forming a long and thick fur, but at most they did not exceed three lines in length. *Rh. tichorhinus* is found in the deposits formed during the diluvian epoch. It is probable that it inhabited Siberia, and the greater part of Europe. This is the species which has been found preserved in the ice of the North of Asia.

The second species is the rhinoceros with nostrils not partitioned (*Rh. leptorhinus*), which had persistent incisors, but concealed in the gums, three toes on each foot, two horns, an elongated cranium, and slender bones. We must unite with it the *Rh. Monspesullanus* of

M. de Serres, and *megarhinus* of Christol. This species, which is not so well characterised as the preceding, has been found chiefly in the superior tertiaries of Italy and the South of France. M. de Blainville likewise refers the bones found in caverns in the South of France to *Rh. tichorhinus*, while those of the North and of Belgium contain only the remains of the preceding species.

The third species is the rhinoceros with incisors (*Rh. incisivus*), characterised by $\frac{1}{2}$ salient incisors in the two jaws, four toes on the anterior feet, flat metatarsi, &c. It would appear that the male bore two horns, and that the female was destitute of these appendages. The latter, for this reason, has been made the type of the genus *Acerotherium* of M. Kaup. The *Rh. incisivus* is found in the middle tertiary formations, and has been described under many names. It will probably be found necessary to refer to it all the species found at Eppelsheim, such as the *R. Goldfussii*, *Schleyermacheri*, *Merckii*, &c., all those found at Sansans, those of Avaray, of Moissac, and Auvergne, described under the names of *elatus*, &c., the *R. minutus* of Cuvier found in the *faluns*, &c.

With regard to the fossil species found out of Europe, M. de Blainville mentions a rhinoceros discovered in the tertiaries of India, and which perhaps does not differ from *R. unicornis*. The pretended rhinoceros of the Alleghanys, has been described from a body which is altogether artificial, and is a gross imposition.

In short, rhinoceroses have not existed during the whole commencement of the tertiary epoch, for the eocene formations yield no trace of them. They have appeared for the first time in the middle or miocene period, during which the *R. incisivus* has inhabited the greater part of Europe. Towards the close of the tertiary epoch, this species has been replaced by the *Rh. leptorhinus*, and during the diluvian epoch, it is the *Rh. tichorhinus* which has been the most abundant and most widely diffused. In the present day, rhinoceroses do not exist in Europe, and are only found in the warmest countries. We find two (perhaps three) species in Africa, one species in Continental Asia, and two in the Sunda Islands. America and New Holland have not any at present, and do not appear to have possessed any in the epoch anterior to our own.

With respect to the zoological relations of the species, we may form them into three groups. The first will contain the *Rh. bicornis* and *simus*, among the living species, and *tichorhinus* and *leptorhinus* among the fossil; the second, the living rhinoceroses of Asia; and the third, the fossil *Rh. incisivus*.—(*Supplement a la Biblioth. Univer. de Geneve*, No. 8, p. 434.)

24. *The Condor of the Cordillera.*—In these steril heights nature withholds her fostering influence alike from vegetable and animal life. The scantiest vegetation can scarcely draw nutriment from the ungenial soil, and animals shun the dreary and shelterless wilds. The condor alone finds itself in its native element amidst these

mountainous deserts. On the inaccessible summits of the Cordillera that bird builds its nest, and hatches its young in the months of April and May. Few animals have attained so universal a celebrity as the condor. That bird was known in Europe at a period when his native land was numbered among those fabulous regions which are regarded as the scenes of imaginary wonders. The most extravagant accounts of the condor were written and read, and general credence was granted to every story which travellers brought from the fairy land of gold and silver. It was only at the commencement of the present century that Humboldt overthrew the extravagant notions that previously prevailed respecting the size, strength, and habits of that extraordinary bird.

The full-grown condor measures, from the point of the beak to the end of the tail, from four feet ten inches to five feet; and from the tip of one wing to the other, from twelve to thirteen feet. This bird feeds chiefly on carrion; it is only when impelled by hunger that he seizes living animals, and even then only the small and defenceless, such as the young of sheep, vicunas, aud llamas. He cannot raise great weights with his feet, which, however, he uses to aid the power of his beak. The principal strength of the condor lies in his neck and in his feet; yet he cannot, when flying, carry a weight exceeding eight or ten pounds. All accounts of sheep and calves being carried off by condors are mere exaggerations. This bird passes great part of the day in sleep, and hovers in quest of prey chiefly in the morning and evening. Whilst soaring at a height beyond the reach of human eyes the sharp-sighted condor discerns his prey on the level heights beneath him, and darts down upon it with the swiftness of lightning. When a bait is laid it is curious to observe the numbers of condors which assemble in a quarter of an hour, in a spot near which not one had been previously visible. These birds possess the senses of sight and smell in a singularly powerful degree.

Some old travellers, Ulloa among others, have affirmed that the plumage of the condor is invulnerable to a musket ball. This absurdity is scarcely worthy of contradiction; but it is nevertheless true that the bird has a singular tenacity of life, and that it is seldom killed by fire-arms, unless when shot in some vital part. Its plumage, particularly on the wings, is very strong and thick. The natives, therefore, seldom attempt to shoot the condor; they usually catch him by traps or by the lasso, or kill him by stones flung from slings, or by the *Bolas*. A curious method of capturing the condor alive is practised in the province of Abancay. A fresh cow-hide, with some fragments of flesh adhering to it, is spread out on one of the level heights, and an Indian provided with ropes creeps beneath it, whilst some others station themselves in ambush near the spot ready to assist him. Presently a condor, attracted by the smell of the flesh, darts down upon the cow-hide, and then the Indian, who is concealed under it, seizes the bird by the legs, and binds them fast in the skin, as if in a bag. The captured condor flaps his wings, and makes ineffectual attempts to fly; but he is speedily secured, and carried in triumph to the nearest village.

The Indians quote numerous instances of young children having been attacked by condors. That these birds are sometimes extremely fierce is very certain. The following occurrence came within my own knowledge whilst I was in Lima. I had a condor, which, when he first came into my possession, was very young. To prevent his escape, as soon as he was able to fly, he was fastened by the leg to a chain, to which was attached a piece of iron of about six pounds weight. He had a large court to range in, and he dragged the piece of iron about after him all day. When he was a year and a half old he flew away, with the chain and iron attached to his leg, and perched on the spire of Santo Tomas, whence he was scared away by the carrion hawks. On alighting in the street, a negro attempted to catch him for the purpose of bringing him home; upon which he seized the poor creature by the ear, and tore it completely off. He then attacked a child in the street (a negro boy of three years old), threw him on the ground, and knocked him on the head so severely with his beak, that the child died in consequence of the injuries. I hoped to have brought this bird alive to Europe; but after being at sea two months on our homeward voyage, he died on board the ship in the latitude of Monte Video.—(*Travels in Peru, by J. J. Von Tschudi*, p. 300.)

25. *Anatomical Researches on the Brilliancy of the Eyes in Certain Vertebrata. By E. Brucke* (*Müller's Archiv*, 1845, p. 387.—M. Brucke, in a preceding Memoir (*Müll. Archiv*, 1844), had endeavoured to discover the use of the *bâtonnets* of the retina. It is known that distant vision results from this, that all the rays of light emanating from a point of space, gaain converge on a point of the retina. In order to satisfy this condition, it is necessary that the luminous rays which have traversed the expansion of the optic nerve should either be entirely absorbed by the choroid, or rather reflected in such a way as again to strike with precision the nervous fibre they have already traversed. The choroid, by its black colour, is eminently fitted to absorb the luminous rays. This absorbing power is probably increased by the inequality of its surface, caused by the presence of transparent *bâtonnets* and their conical prolongations which penetrate between the cells of the pigment. But, in certain vertebrata, we find a tapetum which, far from absorbing the light, reflects it strongly. In this case, these batonnets, arranged as they are perpendicularly to the surface of the retina, serve perhaps to insulate the rays of light, and to direct them, after their reflection from the tapetum, exactly on the same fibres which they have traversed. We can thus understand why the tapetum does not injure vision, and why the animals whose eyes are pro-

vided with it have need of less light than others. The light, in fact, traverses the nervous expansion in animals with a black choroid only once, while it traverses it twice in animals with a tapetum.

The memoir, the little of which has been given above, is a continuation of the first. The author first studies the colour of the reflection from eyes with a tapetum. This colour varies considerably. Seen in the evening by the light of a torch, the reflection in the eyes of a dog passed successively from reddish-brown to fire-red, to blue, green, light yellow, white, and even, in some dogs, to violet. These variations in colour are perhaps owing, in some measure, to the injection of a greater or less quantity of blood into the choroid, but particularly to the changes in adjustment of the animal's eye.

In the Mammifera, the tapetum is a membrane distinct from the choroid. It contains no vessels; it is traversed only by the blood-vessels which unite the two layers of the choroid between which it is found. In Ruminants, Pachyderms, some Marsupials, and Dolphins, the tapetum is fibrous, and composed of undulating fibres, smooth and transparent. But the tapetum of the dog and cat is very different; it is a cellular tissue. It contains no fibres, and is composed of cells more or less hexagonal, and with a transparent nucleus. In the dog the diameter of these cells varies from 0·0008 to 0·0018 of an inch (0·022 to 0·049 millim.)

The author has not found this tapetum in the eye of birds or reptiles. In fishes, the tapetum is composed of cells, in which are arranged crystals, which give it a silvery reflection. These cells are elongated and flattened; their largest diameter is four times greater than that of the tapetum of carnivora.

Lastly, M. Brucke gives the comparative anatomy of the tapetum in the different orders of Mammifera, and fishes in which he found it.

A. L. P.

26. *Memoir Illustrative of the History of the Lamentins of America. By Professor Herman Stannius*, Rostock, 1845, 4to. —In this memoir M. Stannius gives a detailed description of the different parts of the American Lamentin; he has, in particular, collected many interesting facts respecting the osteology, dentition, the form of the nostrils, of the mouth and lips, the larynx and trachæan artery, the vascular system of the periphery and the muscles. Two well-executed plates explain the most important facts.

The form of the cranium leads the author to think that the rivers of America produce at least two species of Lamentin. The first (*Manatus Americanus*, Cuv., *Australis*, Blainv. *Exunguis*, Mus. Vienne) is the smallest, and inhabits the rivers of Brazil, and principally the Amazon. The second (*Manatus latirostris*, Harlan, *Americanus*, Home, &c,), is found in Florida, the Antilles and Guyana.

M. Stannius thinks that the Lamentins, the Dugongs, and the

Stelleres ought to form a distinct order which connects the Pachyderms and Cetaceæ, but which can be united to neither of these.

We ought to add that the figure, given by the author, of the lower jaw of a newly-born Lamentin appears to us very interesting from the obvious resemblance in its form to that of the Dinotherium, although it presents, in the part which is inflected at the base, six sockets of incisive teeth, which are wanting in the fossil genus. In adult Lamentins, the extremity of the jaw partly loses its curvature and proportionate size, and the analogy which we have pointed out becomes less evident.—(*Bibloth. Univ. de Geneve. Supplement*, No. vi., p. 216.)

27. *Memoirs on the Capra Pudu and Equus Bisulcus of Molina. By MM. Gay and P. Gervais* (*Ann. des Sc. Nat.*, February 1846).—Molina, in his essay on the Natural History of Chili, has referred to the genus of goats, and describes, under the name of *Capra pudu*, a small ruminant which systematic authors have had difficulty in referring to its true genus. This animal, named *Venado* by the Spaniards, has successively been regarded as a goat, a sheep, and an antelope. MM. Gay and Gervais shew in this article that it is a small stag allied to the *Cervus rufus* and *nemorivagus*, and that it is probably the same species as the female which lived in the Zoological Gardens of London, and which has been described by Bennett under the name of *Cervus humilis*.

The same work of Molina contains some details respecting an animal which this author names the *Gemul* or *Equus bisulcus*. His description shews that he has no reason for referring it to the horse tribe, and the mammifer indicated by this name is, like the preceding, a true stag, but of much larger size. It approaches the *Cervus antisiensis* found by M. D'Orbigny in the Bolivian Andes, and it appears to form a new species, which the authors of this memoir name *Cervus Chiliensis*. These same naturalists are of opinion that it is necessary to add two others, the *Cervus Spinosus* and *C. Goudotii*, to the list of species now known in South America. M. Gay will describe their characters in the mammalogical part of the History of Chili, which is publishing in the Spanish language.

List of Patents granted for Scotland from 23*d December* 1846 *to* 22*d March* 1847.

1. To EUGENE BAZILE, of Rouen, in the kingdom of France, manufacturer, being a communication from abroad, "improvements in obtaining heat during the manufacturing of coke, and applying such heat to various purposes."—23d December 1846.

2. To JAMES NAPIER, of Shacklewell, in the county of Middlesex,

operative chemist, "improvements in smelting copper ores."—23d December 1846.

3. To **George Fergusson Wilson**, of Belmont, Vauxhall, in the county of Surrey, gentleman, and **John Jackson**, of Southville, Wandsworth Road, in the same county, gentleman, "improvements in the process of, and apparatus for treating fatty and oily matters, and manufacturing candles and night lights."—24th December 1846.

4. To **John Mill M'Pherson**, manager, Greenhead, of Glasgow, in the county of Lanark, Scotland, "certain improvements in weaving."—28th December 1846.

5. To **William Little**, of No. 198 Strand, publisher of the Illustrated London News, "improvements in machinery for printing."—30th December 1846.

6. To **Henry Mapple**, of Child's Hill, Hendon, in the county of Middlesex, machinist, "improvements in apparatus for transmitting electricity between distant places, and in electric telegraphs."—30th December 1846.

7. To **Charles Payne**, of Whitehall Wharf, Cannon Row, Westminster, gentleman, "improvements in preserving vegetable matters."—30th December 1846.

8. To **David Davies**, of Wigmore Street, Cavendish Square, in the county of Middlesex, coach-maker, being a communication from abroad, "certain improvements in steps for carriages and other purposes."—30th December 1846.

9. To **Adrien Chenot**, of Clichy, La Garene, near Paris, in the kingdom of France, formerly a student in the Royal School of Mines in France, "certain improvements in the treatment of metallic oxides and their compounds, and in apparatus for the same."—31st December 1846.

10. To **Stephen R. Parkhurst**, of Leeds, in the county of York, manufacturer, "improvements in carding wool, cotton, and other fibrous materials."—31st December 1846.

11. To **Thomas Morton Jones**, of Birmingham, gentleman, "improvements in heating liquids and æriform bodies."—31st December 1846.

12. To **Alexander Bain**, of 11 Hanover Street, Edinburgh, electrical engineer, "certain improvements in transmitting and receiving electrical telegraph communications, and in apparatus connected therewith."—5th January 1847.

13. To **John Watson**, of Glasgow, manager to Messrs Gilmour and

Kerr, power-loom cloth manufacturers, "improvements in weaving by jacquard looms by power."—5th January 1847.

14. To William Air Foster, leather-merchant and bootmaker, in Glasgow, in the county of Lanark, "an improved mode of making belts (for driving machinery and for other like purposes), traces, reins, and other articles of leather, pelt, or parchment, and also certain apparatus or machinery therein applicable."—7th January 1847.

15. To Henry Constantine Jennings, of No. 6 Cumberland Terrace, Regents Park, in the county of Middlesex, practical chemist, "a new method, or apparatus, or machine, for the better or more economic evaporation of fluids or liquids containing crystaline or other matters to be concentrated or crystalized."—12th January 1847.

16. To Lionel Campbell Goldsmid, of Rue Magador, Paris, Esq., being a communication from abroad, "improvements in applying rudders to ships and other vessels."—21st January 1847.

17. To John Buchanan, of Queen Square, Westminster, gentleman, "certain improvements in the construction of ships or vessels, and in propelling thereof."—21st January 1847.

18. To Samuel Canliffe Lister, of Manningham House, in the parish of Bradford, in the county of York, Esquire, "improvements in preparing and combing wool."—25th January 1847.

19. To Edward Cobbold, of Millford, in the county of Suffolk, clerk, M. A., "improvements in the preparation of peat, rendering it applicable to several useful purposes, particularly to fuel."—25th January 1847.

20. To Matthew Gibson, of Wellington Street, in the borough and county of Newcastle-upon-Tyne, machine-maker, "a machine for reaping, cutting grass, and other similar purposes."—26th January 1847.

21. To Henry Bessemer, of Baxter House, Old Saint Pancras Road, in the county of Middlesex, engineer, "certain improvements in the manufacture of glass, and in the machinery and apparatus connected therewith, and also in silvering and coating glass, parts of which improvements are applicable to the manufacture of tin-foil, and thin sheets of other metal or alloys of metal."—28th January 1847.

22. To John Thompson Carter, of Drogheda, in the county of the town of Drogheda, in Ireland, flax-spinner, "improvements on machinery for crushing, bruising, and preparing flax, hemp, and other fibrous materials requiring such treatment."—29th January 1847.

23. To Richard Walker, of Rochdale, in the county of Lancaster,

over-looker, "certain improvements in looms for weaving."—15th February 1847.

34. To Juan Nepomuceno Adorno, of Mexico, in the Republic of Mexico, gentleman, "improvements in manufacturing cigars, and other similar articles."—22d February 1847.

35. To William Eaton, of Camberwell, in the county of Surrey, engineer, "improvements in machinery for twisting cotton, or other fibrous substances."—23d February 1847.

36. To Alfred Krupp, principal of the house of Frederick Krupp, of Essen, Prussia, but now of Leicester Square, in the county of Middlesex, "certain improvements in the manufacture of spoons, forks, and other similar wares, and in the machinery or apparatus employed therein, parts of which are also applicable to other manufacturing processes."—24th February 1847.

37. To William George Armstrong, of Newcastle-upon-Tyne, Esquire, "an improved lifting, lowering, and hauling apparatus."—25th February 1847.

38. To Charles Barlow, of the Patent Office, 89 Chancery Lane, in the county of Middlesex, gentleman, being a communication from abroad, for "a new apparatus and arrangement of machinery to be used in collecting the contents of sewers, drains, and cess-pools, and also in treating the mass so collected in such a manner as to render the same applicable to agricultural and other useful purposes."—26th February 1847.

39. To Thomas Hutchison, of Paisley, Scotland, salesman, "a machine for constructing patterns in stripes, checks, and tartans for woven and other fabrics."—26th February 1847.

40. To Pierre Armand le Comte de Fontainemoreau, of 15 New Broad Street, City, English and Foreign Patent Office, being a communication from abroad, "certain improvements in the machines for the manufacture of bricks and other plastic products."—2d March 1847.

41. To Andrew Crosse, of Broomfield, in the county of Somerset, Esquire, "improvements in treating fermentable and other liquids, so as to cause impurities or matters to be extracted or precipitated."—4th March 1847.

42. To Francis Stanilas Meldon de Sussex, of Millwall, in the county of Middlesex, manufacturing chemist, "improvements in the manufacture of chlorine hydrochloric acid and nitric acid, and obtaining several products therefrom."—4th March 1847.

43. To Henry Henson, of Hampstead, in the county of Middlesex, gentleman, "a new fabric, suitable for goods, wrappers, waggon-covers,

and other like purposes; and certain processes employed in the manufacture of the same."—5th March 1847.

44. To Robert Stirling Newall, of Gateshead, Esquire, "certain improvements in locomotive engines."—5th March 1847.

45. To John Wood, of Leeds, in the county of York, machine-maker, "certain improvements in machinery for spinning fibrous substances."—8th March 1847.

46. To George Lowe, of Finsbury Circus, in the county of Middlesex, civil-engineer, "improvements in the manufacture of and in burning gas, and in the manufacture of fuel."—9th March 1847.

47. To James Roose, of Durlaston, in the county of Stafford, tube-manufacturer, "improvements in the manufacture of welded iron tubes."—9th March 1847.

48. To Charles Richardson, in the county of Middlesex, being a communication from abroad, "certain improvements in making and refining sugar, and in the application of the products of the sugar-cane to manufacturing purposes, and also in the machinery and apparatus employed therein."—10th March 1847.

49. To Albert Robert Cunningham, of Sydenham, in the county of Kent, gentleman, and Joseph Threlfall Carter, of Sydenham aforesaid, engineer, "certain improvements in propelling carriages on railways."—22d March 1847.

50. To William Newton, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, civil-engineer, being a communication from abroad, "certain improvements in engines to be worked by gas, vapour, or steam, either separately or in combination."—22d March 1847.

51. To Charles Fox, of No. 3 Trafalgar Square, in the county of Middlesex, engineer, "improvements in the construction of presses in shearing, cutting, or punching pieces of metal in welding or uniting pieces of metal together, and in pressing or forming pieces of metal into forms or shapes."—22d March 1847.

NOTICE.

Several articles, as Notices of New Publications, owing to confusion in the Printing-Office, caused by a *strike* of the printers, have been necessarily postponed until our next. We may however mention, that the American, British, French, German, and Indian Periodicals have come to hand. Also Mr Darwin's lately published valuable Geology of South America; the 2d volume of Professor Owen's Classical Hunterian Lectures, and Mr A. Henfrey's very interesting "Outlines of Structural and Physiological Botany," in one volume 12mo., which will prove a good text-book for private study, and also for pupils attending lectures on Botany.

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END OF VOLUME FORTY-TWO.

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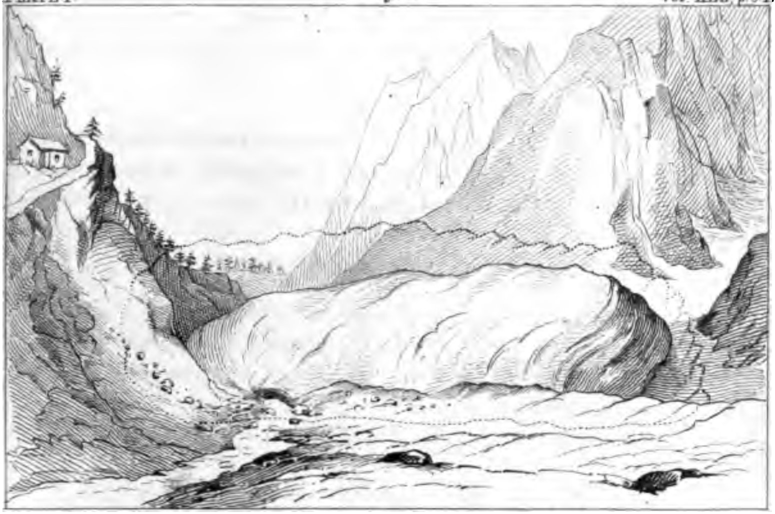
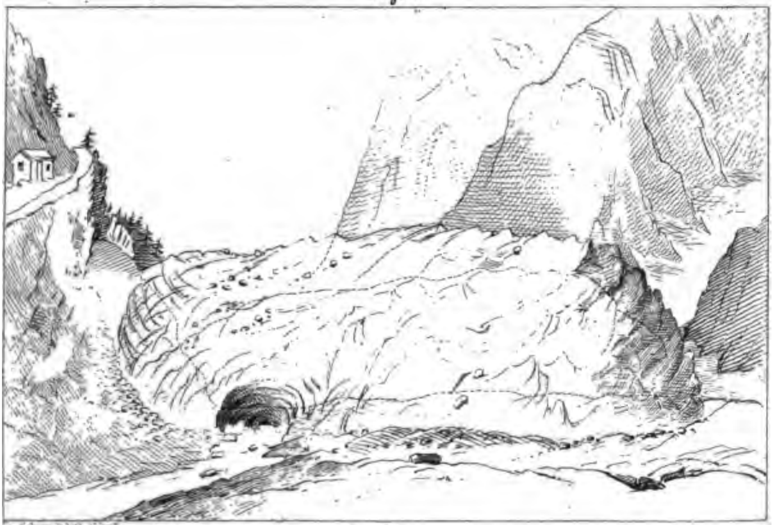
PLATE I. Fig. 1. Vol. XLII, p.94.



Glacier of la Brenva in 1842.

Fig. 2



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Glacier of la Brenva in 1846.

PLATE IV.

Edinr. New Phil. Jour. Vol. XLII. p.281.

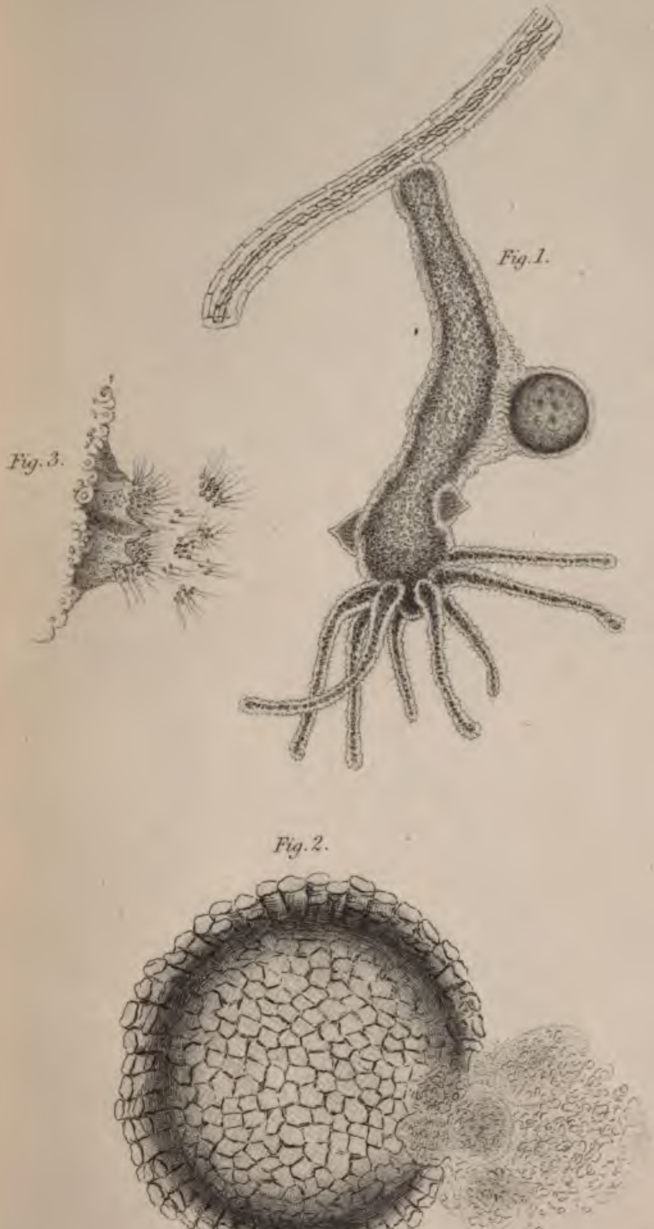
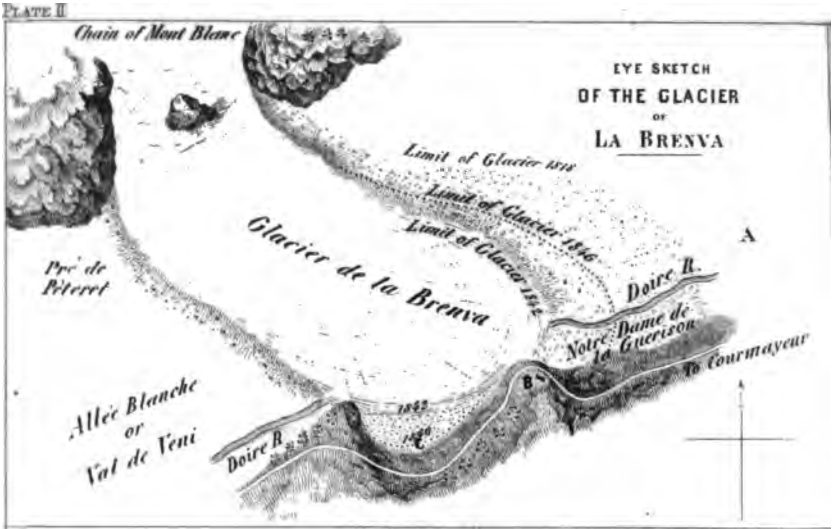


Fig. 1.
Fig. 3.
Fig. 2.

Ovum and Spermatic Capsules of Hydra viridis.



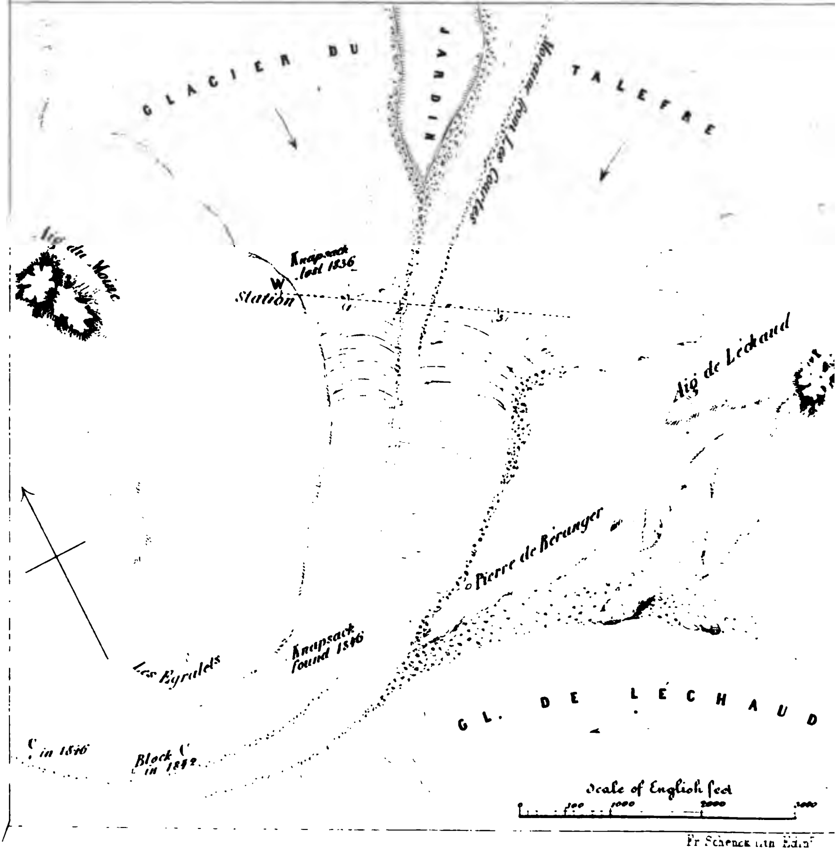


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Fig. 3.
Section

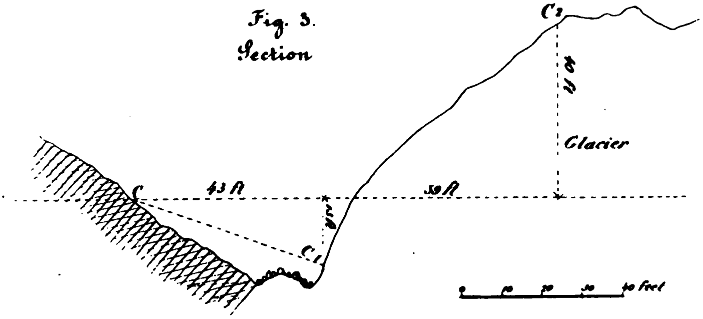
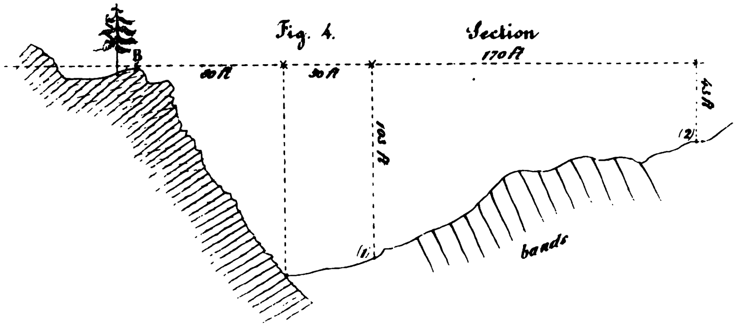


Fig. 4. Section



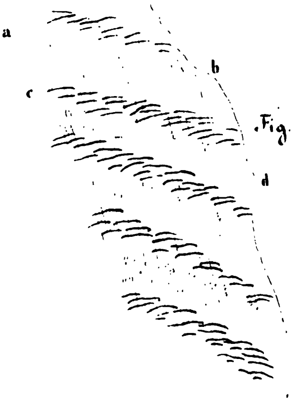
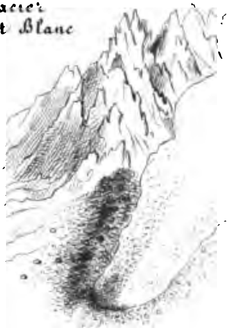


Fig. 5.

Fig. 6.

Glacier du Nant Blanc



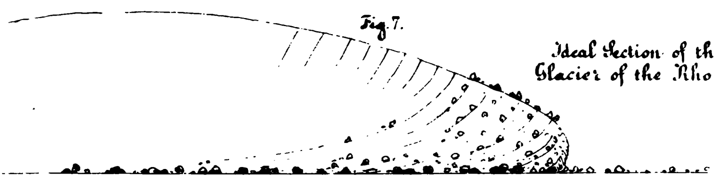
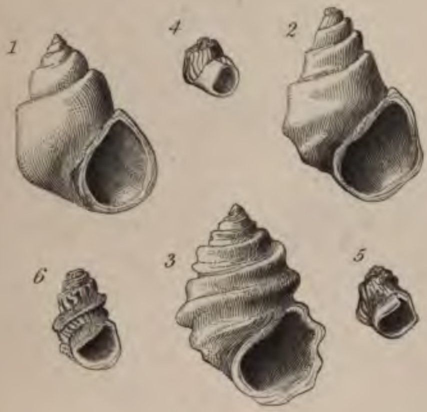


Fig. 7.

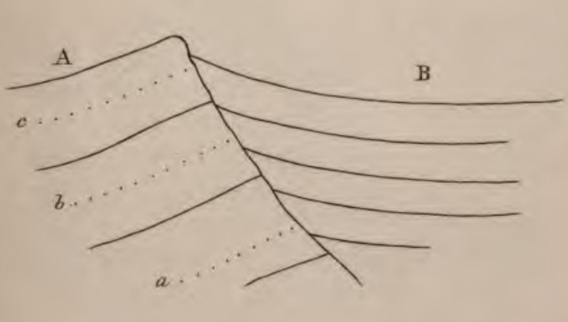
Ideal Section of the Glacier of the Rhone

PLATE III. Edinʳ New Phil. Jour. Vol. XLII p. 2

TERTIARIES OF THE ISLAND OF COS.



1. *Paludina of lowest horizon: 2. of the second, and 3 of the uppermost zone. 4, 5, and 6, successive forms of Neritina.*



Tertiaries of Cos. A, freshwater beds, a, b, c, successive series of fossiliferous zones. B, Marine Newer Pliocene beds.

J. G. Mitchell, Sc.

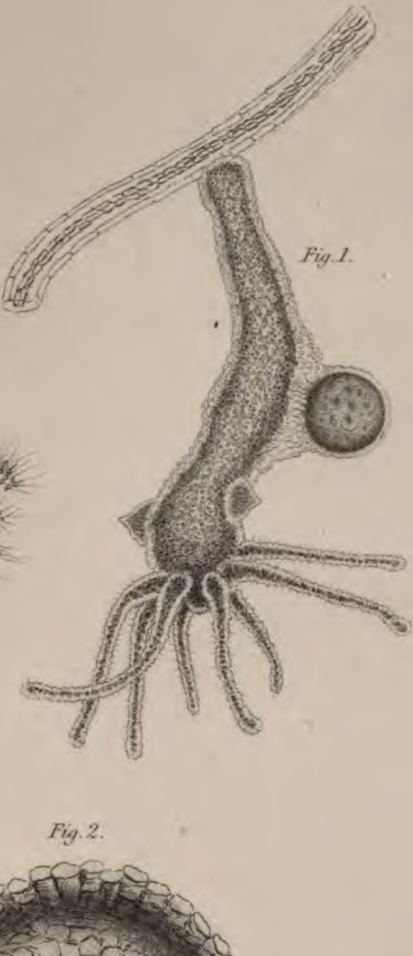


Fig. 1.

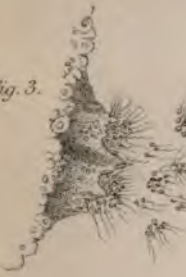
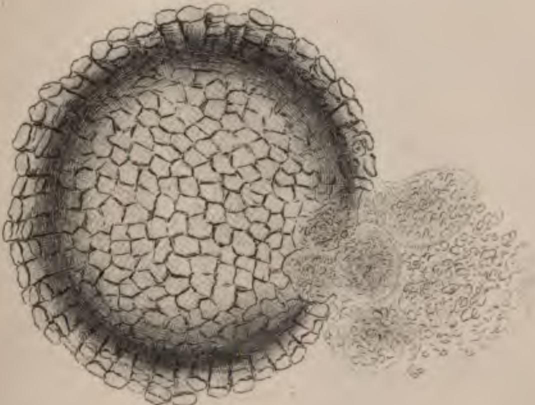


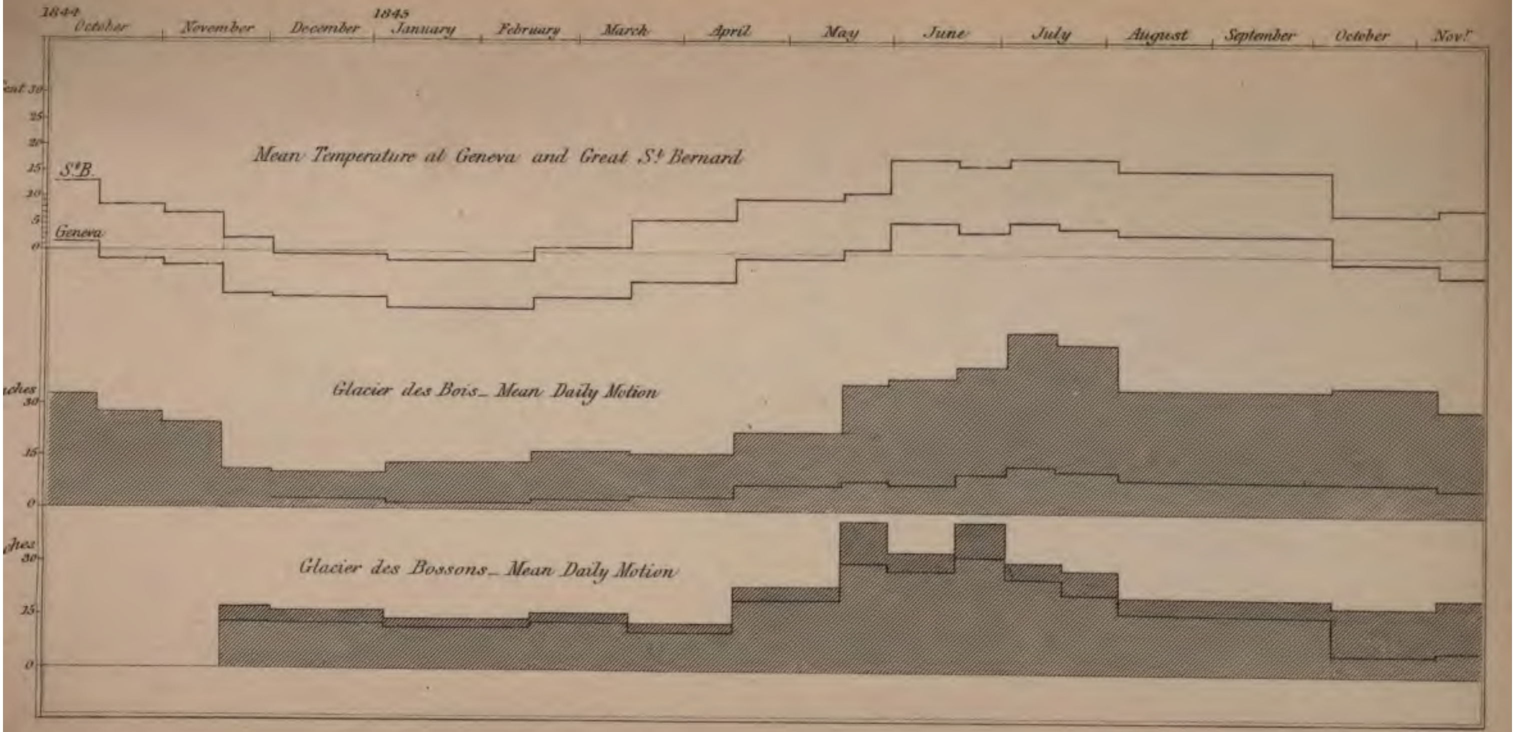
Fig. 3.

Fig. 2.



Ovum and Spermatic Capsules of Hydra viridis.

page 327.



J. G. Mitchell, Sculpt

AN

EXAMINATION

OF THE

ASTRONOMICAL DOCTRINE

OF THE

MOON'S ROTATION.

"Strike! but hear me."

EDINBURGH:
ADAM AND CHARLES BLACK;
ONGMAN, BROWN, GREEN, AND LONGMANS, LONDON.

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ASTRONOMICAL DOCTRINES, &c.

MODERN astronomers seem all to be agreed in opinion that the moon rotates, or turns round on her axis, during the period of her revolution about the earth. They speak of it as an indubitable fact; and, without taking the trouble to examine it, I was induced by their unanimity of opinion, and the authority of their great names, to acquiesce in it as a matter of faith, though inclined to be very sceptical about its truth. Some time ago, however, my attention was accidentally called to this subject, my doubts of its truth became stronger than ever, and I resolved to probe the matter to the bottom. The result is a decided conviction that my faith has been misplaced, and that the moon does not rotate, or turn round on her axis, in the manner alleged. The investigation led me even further than I anticipated; for it suggested a consideration of the established doctrine concerning the sidereal day, and the result of my inquiry into that subject also is the same. The problems are so simple that very few words might suffice to show how they are to be solved; but the received opinions have prevailed so long, and have become so deeply engrained in the minds and systems of astronomers, that it is necessary to examine in detail their arguments and illustrations, in order to show fully and distinctly the nature, the cause, and the absurdity of their errors.

Professor Nichol of Glasgow, in his treatise on "*The phenomena and order of the Solar System*," says, "The moon has one very singular feature, apparently belonging essentially to her as a satellite, since it distinguishes every other satellite with which we are sufficiently acquainted. *She rotates on her axis in the same time in which she revolves in her orbit round the earth*,—in other words, the length of one day and night in that orb is precisely one of our months. The rotations of Jupiter's satellites also nicely correspond with the periods of their revolution around that planet, and all we know or suspect of the attendants on Saturn is in obedience to the same law. Why this is so is unknown—it is one of those dark points that stir us to profounder inquiry concerning the scheme in which we are; but it is not difficult to trace some of its effects. *Owing to this arrangement the moon always turns the same face towards the earth.* As she goes her monthly rounds, she revolves proportionally on her axis—each spot keeping its relative position towards us; and if it were not that her velocity in her orbit is slightly irregular, we should never see anything of her opposite hemisphere," p. 159. Other astronomers state the doctrine in very similar terms, but perhaps the best and most explicit expression of the received doctrine of lunar rotation is that given by *M. S. D. Poisson* in his *Traité de Mecanique, 2de edition, Tome II. p.* 179–180, in these words: "Le mouvement de translation peut etre revolutive autour d'un autre corps en repos, ou lui meme in mouvement. Il n'y a pas de rotation toutes les fois qu'une face ou une section determine du mobile reste constamment parallele a elle meme; il y a, au contraire, rotation dans le meme temps que la revolution, lorsque le mobile tourne constamment la meme face vers le corps central. C'est ce second cas que a lieu dans le mouvement des satellites autour de leurs planètes. La lune tourne toujours la meme face vers la terre, et le rayon vecteur qui va du centre de la terre au centre de la lune rencontre toujours en un meme point la surface du satellite, d'ou il resulte que la rotation de la lune sur elle meme, et sa revolution autour de la terre s'achevent dans un meme temps." "The movement of translation (of a progressing body) may be revolutive round another body at rest, or itself in motion. *There is no rotation* when one face or a determinate section of the moving body remains constantly parallel to itself; *there is*, on the contrary, *rotation* in the same time as the revolution when the moving body turns constantly the same face towards the central body. This second case is that of the satellites in moving around their primaries. The moon turns always the same face to the earth, and the radius-vector, which connects the earth's centre with the moon's, cuts the surface of the satellite always at the same point; whence it results that the moon's rotation on her own axis, and her revolution about the earth are accomplished in the same time."

The question at issue is simply this: astronomers say that it is possible for the moon, or any other body revolving in an orbit, to travel round that orbit, and present every part of its circumference in succession to the orbit's centre, without turning round on its own axis; that the moon, revolving freely in space, should exhibit this phenomenon; but as she does not, and, on the contrary, presents to the earth the same face continually, it is necessary that to enable her to do so, and *bring* continually the same point *back* to the radius-vector, she must rotate, or turn round once on her axis during each revolution in her orbit, in a direction *contrary* to that of her revolution. In opposition to this, I have seen reason to believe, and shall endeavour to prove, that the alleged presentation of every part of the circumference, *without rotation* or turning round, is impossible; that the shuffling kind of progression to which is ascribed this impossible effect, is a real retro-rotation or turning round on the axis backwards, in a direction really *contrary* to that of the moon's revolution, namely, from east to west; that the rotation which they ascribe to the moon is not "contrary to," but in the *same direction with*, that of her revolution, namely, from west to east; and that, were she to rotate once in that direction during each orbital revolution, the inevitable result of such a rotation would be the presentation, not of the same point continually, but of every part of her circumference in succession to the centre of her orbit. I admit, however, that if the moon were indeed to move freely in space, she would move in the manner they allege, and which I call retro-rotation; and that, as she does not, there must be some power acting against her freedom, and producing the effect of *keeping* the same face continually looking to the earth; but that, whatever may be the nature of that power, its effect is *not to cause* but to *prevent* the moon from rotating "in a direction contrary to that of her revolution;" in short, to keep her from rotating at all. She does, indeed, turn round about in presence of the sun, and *without turning round about* in some way or other, neither the moon nor any other body revolving in an orbit could present every part of its circumference as the moon does to the sun or other fixed object, or stationary spectator, outside, or indeed to any other body or spectator whatever; but so far is the moon's turning round from being a revolution of her circumference round about her axis, that, on the contrary, it is the axis itself, carrying the whole mass of her body forward along with it, that virtually revolves about a pivot on the radius-vector, close to the innermost point of circumference, or that point itself, as its centre of revolution. To the sun or other spectator outside, the effect of this kind of revolution is the same as would be the effect of the moon's turning once round on her axis, while remaining at a fixed point of the orbit; but, with respect to the earth or the centre of the orbit, the presentation of every point of the revolving body's circumference without actual rotation or turning round on the axis is impossible, so long as the axis itself revolves at a fixed distance from the centre of its orbit. If, however, the body were to remain at a fixed point, and turn round with its whole mass, including the axis, about that point, as its centre of revolution, then of course its whole circumference in succession would be presented to the earth as well as to the sun; but in a regular orbital revolution, as that of the moon or the earth, the presentation of the whole circumference to the orbit's centre, without the revolving body's turning round on its axis, is perfectly impossible. Were a carriage wheel to slide along a rail with the same point of its circumference continually in contact with the rail, would any one ever imagine it was all the time turning round on its axle? With just as little propriety and show of reason do astronomers affirm that, when the moon slides along the rail of her orbit with the same face continually looking to the earth, she not the less turns round on her axis. For, whether the progressive movement be made along a straight line or a curve, the line of motion described by the progressing axis is always and invariably parallel to, or concentric with, the tracks or lines of motion described by the diametrically opposite points of its circumference, each at the angular distance of 90° from those points of circumference intersected by the orbit of the axis, and a line drawn through the axis, directly across, and perpendicular to these three lines of motion, and through those diametrically opposite points, will always measure and indicate the body's rotation. Such a line is the radius-vector of the moon or other body revolving in an orbit; and it is utterly beyond the possibility of any *reasonable* question, that a revolving body, which keeps the same point of its circumference continually attached to the radius-vector, in precisely the same relation to the axis and all these other points, must revolve without rotating or turning round on its axis. It is, indeed, nothing less than a positive contradiction in terms to say that the moon *rotates*, when, as admitted, she really *does not turn round about on her track like a wheel;* for, without turning round about on her track *like a wheel*, she cannot turn round about on her axis. The one is the indispensable condition and accompaniment of the other.

Dr Johnson defines "rotation" to be "the act of whirling round like a wheel; the state of being so whirled round; whirl; vicissitude of succession." Well; how do the phenomena of the moon's motion correspond with these definitions? She presents continually the same face to the centre of her orbit; and astronomers allege that phenomenon to be the effect of her *rotation*, or whirling round like a wheel on her axis *once* during the period of her orbital revolution; but, strange to say, this alleged rotation, or whirling round, so far from producing a "vicissitude of succession" in respect of the fixed centre of her orbit, leaves the same point of circumference constantly in the same relative place towards that centre, just as if there were no rotation or whirling at all. The moon maintains precisely the same relation to her orbit and its centre, no part of her circumference passing the one or crossing the other during the whole revolution, so as to shew the indispensable signs of rotation, or whirling round on an axis. And to make the matter worse, it is alleged that every "vicissitude of succession" in the parts of the moon's circumference, with respect to the earth, might be produced *without rotation,* thereby completely confounding and running counter to the great lexicographer's definition, as well as to common sense. By "rotation," or turning round on an axis, is universally understood to be meant the fact of a body's moving all the parts of its circumference round about an axis within itself, presenting them all in succession on every side of that axis, and making them all in their turn pass a straight line, real or imaginary, connecting that axis with a given point outside, perpendicular to the body's line of motion; or, in other words, crossing the line of motion at right angles; such as, in the moon's case, the radius-vector that connects her axis with the centre of her orbit, and which, if her orbit were a perfect circle, would be always perpendicular to her line of motion, and cross her orbit at right angles, at whatever point of that orbit the moon might be for the time; or, as in the case of a common wheel, a rod crossing it diametrically, perpendicular to the road or rail along which it is moving, or a wheel moving in a frame with which its axis is connected by a cross bar. But, when the moon *rotates*, as is maintained, so far is she from moving all the parts of her circumference round about her axis, presenting them all in turn on every side of that axis, and making them in succession pass her radius-vector or cross-bar, that, on the contrary, she keeps them all invariably on the same side of her axis, and the same three points of her circumference severally attached to the radius-vector, and to the orbit at either side, 90° from that radius; while the inner end of the radius-vector is virtually attached to a fixed point which never changes its place, so far at least as the moon is concerned. If the radius were loose, so as to be at liberty to move past the orbit's centre, it might be considered as virtually a portion of the moon's substance, and consequently as partaking of all the moon's movements; and, in that case, there might be some show, but only a show, of truth in the allegation. But when it is considered that the radius is, on the contrary, virtually a part of the centre of the orbit itself, merely an extension or expansion of that centre, and nothing more than the representative of the several points of its circumference in succession at the place where the moon passes them, even the show of truth is gone, and the doctrine stands forth in its glaring absurdity. Moreover, astronomers affirm, that if the moon were to move the parts of her circumference backwards, or from east to west, across the orbit outwards in that direction, and back again on her other side, presenting them all in their turn on every side of her axis, and making them all in succession pass her radius-vector, thereby exhibiting to the earth a complete "vicissitude of succession," and all the signs of rotation, or whirling round on an axis, all this would or might be produced without rotation, or turning, or whirling round! It is also stated, that not only the middle point of that side of the moon which is always looking to the earth, but also the radius-vector and *the earth itself*, or at least "its visual ray," which is the same thing, revolve (that is, move in an orbit, or describe a circle,) round about the moon's centre, while that very centre itself is revolving about the earth! Paradoxical as such doctrine seems, and well calculated as it is to startle and surprise, and provoke to scrutiny, it has nevertheless kept its ground for two centuries, and received the solemn sanction of the great minds of whose achievements astronomy so proudly boasts. But how will unscientific readers be amazed to be told that all these paradoxes are founded on no less monstrous an assumption, than that it is the property of the centre of a circle not only to change its place continually within the circle, but even to rotate or go round about the axis of a body moving along the circle's circumference, yet *without going out of the circle*, which one would think an indispensable requisite to enable it to perform that rotation! Without this fundamental proposition to rest on, the doctrines of the moon's rotation, and of the sidereal day's being the true measure of the earth's rotation cannot subsist a moment. This will be best accomplished, perhaps, by stating the simple truth, which must at once receive the assent of those who examine the question, even though they be told at the same time that the opposite error has been expressly affirmed by Galileo and La Place, sanctioned by Newton, and tacitly at least, perhaps inadvertently, admitted by all other astronomers, as an established truth in their science.

The centre of a circle, considered in relation to the circle itself, is a fixed point, which remains invariably in the same place, equidistant from every point of the whole circumference; and, consequently, every radius, or straight line drawn from the centre to the circumference, will be invariably of the same length, and cut or touch the circumference at right angles.

From this it follows obviously that every point of the circumference, and all the points together, whether limited in number, or infinitely numerous, will bear precisely the same relation to the centre. Each of them will occupy the intersection of two lines crossing at right angles, or perpendicular to each other, namely, the radius that connects it with the centre, and the circumference itself, of which it forms a part.

The centre of a circle is, by the necessity of its nature as such, incapable of changing its place within the circle, and, consequently, maintains invariably the same relation to every part of the circle and its circumference, and to every body, axis, or mathematical point, moving about on that circumference. At whatever point of the circumference the revolving axis may be; however much it may vary its relations to objects outside, *its relation to the centre is invariable*, namely, it is always at the outer end of the shortest straight line that can be drawn between it and the centre, and which straight line, invariably perpendicular to the circumference of a perfect circle, is its radius-vector. Each of the radii also, whether considered as lines in the substance, or on the flat surface of the circle, including the flitting radius-vector itself, is by the same necessity of its nature as the radius of a circle, just as incapable as the centre itself of changing its angular relation to the circumference and the centre, and remains invariably the shortest straight line between them at any given point. Whatever, therefore, can be truly predicated of any movement or event taking place at the outer end of any one radius, would be just as truly predicated of the same or a similar movement or event if it were to happen at the circumference, at the outer end of any other radius, or proportionally at the outer end of each of them in succession.

When a round body, as a globe or wheel, moves forward in space, the direction of its line of motion is indicated by a line, straight or curved as the case may be, passing through its axis, parallel to, or concentric with, the road, rail, or mathematical line traced by either side of its circumference; and if it rotate or turn round on its axis, the rotation is indicated by the passage of every point of its circumference in succession past that line of motion, and also past another line drawn perpendicularly to the line of motion, and which perpendicular line, in the case of orbital progression and revolution, is the radius-vector, virtually identical with any one, and with all in their turn, of the fixed radii of the orbit, and as incapable, by the necessity of its nature, as any one of the fixed radii themselves, which it successively represents, of turning round about on the axis of the revolving body. If, on the contrary, no part of the revolving body's circumference pass the radius-vector and the line of motion, the same respective points remaining continually in one with one or other of these lines without any change of their relation to either of the lines, then it cannot be more certain that the sun and the moon revolve, or seem to revolve, about the whole heavens, than that a body so remaining unchanged in all its relations to its own line of motion and radius-vector, progresses without turning round on its axis. The moon, however, by the admission and declaration of all the astronomers of the last two centuries, revolves in her orbit precisely in these circumstances; that is, without carrying the points of her circumference in succession past her radius-vector, and across the line of motion described by her axis, and, consequently, keeping the same face continually looking to the earth, and very nearly the same visible points of her circumference continually in one with her radius-vector; and no logic can evade the conclusion, deduced from the very nature and circumstances of all rotation, that she revolves in her orbit *without turning round on her axis.*

The radius-vector of a planet, or other body moving round an orbit, may be considered in two points of view: either as a constituent and substantive part of the orbit itself, or of its centre, or as a constituent and substantive part of a body revolving in contact with the centre of its orbit. In the former case, the radius-vector is virtually a rod attached by a hinge or ligature to the centre of the orbit, or, more correctly still, it is virtually a line drawn on the surface of a disk co-extensive with the orbit, and consequently capable of making no more than one revolution round only one of its ends at once; and the end round which only it is possible for it to revolve is the one attached to the centre of the orbit, and no more movable from its place than the centre itself is. And, in performing that single revolution of which alone it is capable, each of the points of which every radius is composed moves with different degrees of velocity, proportional to its distance from the centre, and all these points moving together describe a series of concentric circles, increasing in dimensions as they go outwards. Consequently, all the points move through unequal spaces in equal times; and the result is, that, while the innermost points describe an invisible circle round an invisible point, all the other points describe always increasing circles round that point as well as the centre; whereas, on the contrary, according to the astronomical doctrine I am combating, each of these points describes a circle, *not round the centre itself, but round the outer end of the radius of which it forms a part,* while that outer end, and that only, by itself, describes a single circle round the centre of the orbit.

In the other point of view, the radius-vector may be considered as a part of the revolving body, and to represent by its inner end the circumference of a circle as large as the orbit itself, but only half within the orbit, and having the axis of the revolving body for its centre. But it is obvious that the radius-vector so circumstanced, so long as it remains attached to the orbit's centre, can no more revolve about its outer end, or the axis of the revolving body, than any of the radii of the solid disk we have been considering, or than the centre itself; and, consequently, the point of the revolving body's circumference that remains always in one with the radius, can no more revolve about the axis than the radius which accompanies it; and, on the contrary, were that point of circumference really to revolve about the body's axis, accompanied by the radius-vector, the indispensable condition of such a revolution would be the setting of the radius-vector free from its attachment to the orbit's centre, and then its inner point, so set free, would be seen moving away from the centre to the outside of the orbit, and back again, just as often as the body turned round on its axis, whether that happened *once* only, or twice, or a thousand times during a single revolution in its orbit.

But the radius-vector is not a material vinculum capable of being thus detached and moving away from the centre. It is merely a mathematical or imaginary line, forming the shortest straight line between the revolving body's centre and the centre of the orbit; and this character it preserves at every point all round the orbit's circumference. But these points of circumference remain invariably at the same distance from the centre; and the centre or axis of the revolving body has precisely the same relation to that centre as the point of the orbit which it may occupy for the time, so that its radius-vector is successively identical with, or occupies precisely the same place as, the radii one after another of the points which it passes, and is consequently invariably perpendicular to the planet's line of motion, or the orbit which the planet's axis describes; and round that axis it could not revolve without losing its perpendicularity, and ceasing to be the radius-vector, or, simply, could not revolve at all. If a material vinculum, acting as a radius-vector, were to be set free from attachment to the orbit's centre, it would then become virtually an outlying arm of the planet itself, partaking of all its movements, whether of orbital revolution or rotation; whereas, on the contrary, the true radius-vector, by the necessity of its nature as such, takes part in the orbital revolution only, and is never, and cannot be, in the slightest degree affected by the planet's rotation on its own axis. The true immaterial radius-vector remains invariably, in all circumstances, the shortest straight line between the revolving axis and the immovable centre round which it revolves. It follows of course the forward movement of the revolving axis, but never does nor can revolve about it, any more than the centre itself of the orbit, which is certainly fixed in its place and immovable. If, then, the invariable rectangular radius-vector cut invariably the same point of the planet's circumference, that of itself is the most direct, the surest, and the most incontrovertible proof that the revolving body does not turn round on its own axis; for to say that it does so, in such circumstances, is no less preposterous than to uphold that the immovable centre of the orbit leaves its place, and revolves round about the moving axis, at the very time that axis is revolving about the centre itself.

The axis of a revolving body remains constantly at the same distance from the centre of its orbit, and the radius-vector is always the shortest straight line between that axis and that centre. But the radius-vector has only *one* movement, always *forwards* with the moving axis which it connects with the immovable centre, its different parts moving with different degrees of velocity proportional to their distance from the centre. It never *turns back* its inner end any more than the centre itself to which it is attached turns back from its central position, and consequently every change that takes place in the relation of the parts of the moon's mass or circumference to that radius can only be the effect of movement in the moon itself or its parts. The relation of the radius-vector to the moon's axis is invariable; and if the same point of the moon's circumference be always intersected by the radius, the relative position of that point not only to the radius, but also to the axis of the moon and to the centre of the orbit, must be just as invariable as the invariable and immovable centre itself. It will always keep the same relative place on the shortest straight line between the axis and the centre, being itself the nearest point of the revolving body to that centre; and there is only one way in which its relative position can be changed, namely, by its diverging from the radius-vector, either to the right hand or to the left. In either case the moon's mass will oscillate, or turn on its axis, to the extent of the divergence, and if the divergence be continued in either direction till every point of the circumference in succession shall have passed the radius-vector, the body will have performed a complete rotation on its own axis, whatever may be the phenomena or appearances which it may present in other respects. The passing of all its points across the radius-vector is the surest, as it is indeed the most perfectly incontrovertible sign and proof of its rotation.

The radius-vector certainly does not cross the orbit with it; the inner end of the radius-vector, which, I may say again, is nothing else than the shortest straight line betwixt the moon's axis and the centre of the earth, certainly *does not revolve* in the opposite direction round about on its outer end at the moon's axis, unless it were true, what is, however, positively and perfectly impossible, that the fixed centre itself revolves about the moon and her axis. How, then, is it possible to avoid the conclusion that, if the moon were to revolve with all the points of her circumference in succession *passing the radius-vector and crossing the orbit*, though she might at the sametime keep the same face constantly looking to the same fixed point outside; or, in other words, supposing her to move in or on a horizontal plane with the north, south, east, and west quarter points of her circumference always directed, each to the corresponding point of the horizon, she could only accomplish that feat by means of a rotation, or turning round on her axis *once,* during the period of her orbital revolution? In such a case, one of two things must happen; either the point of the revolving body that diverges from the radius-vector westward turns round on the body's axis 90° in each quadrant, or the centre of the orbit, carrying the inner end of the radius-vector along with it, turns round about the body's axis to that extent in the contrary direction. But the centre is a fixed spot, which, by the necessity of its nature as a centre, is incapable of moving from its place; the radius-vector, proceeding from that centre, is by its nature just as incapable as the centre itself of turning backwards by its inner end; while, on the contrary, not only is there no incapability of movement on the part of the planet, but there has been a real and visible movement of the parts of its circumference, past and away from the radius-vector and the centre and across the orbit; therefore, it follows inevitably, that the divergence to be accounted for is and can only be the effect of the last mentioned movement of the planet, and of a real and actual rotation, or turning round, of the body on its axis.

From these premises it follows inevitably that whensoever, and to whatsoever extent there happens a change of relations between the parts and points of circumference of a body revolving in an orbit, or circular path, on the one side, and the radius-vector or other radius, or radii of the circle and its centre, on the other, that change must be the result, solely and exclusively, of a rotary movement of the revolving body on its own axis. For example, suppose a disk to be placed at the north point of an orbit, and to turn once round on its own axis at that point, every point of its circumference in succession would be seen to pass the radius-vector, till they had all completed a rotation about the axis, the point that first diverged from the radius on the one side being the last to return to it on the other. But if, instead of completing the rotation at N, the planet were to rotate to the amount of just one degree of its orbit, the result would necessarily be precisely the same, so far as respects the radius-vector, and the invariable centre which the radius everywhere represents; and that, whether the rotation be from west to east, or from east to west. If a cord were attached to the circumference of the disk and to the centre of the orbit, it would be carried round about the axis precisely in the same way, whether the rotation were performed all at once at N, or only gradually during the revolution; and such perfect sameness of effect can only be reasonably ascribed to sameness of cause. When, therefore, the planet moves one degree, and each point of her circumference at the same time diverges one degree from the radius, that divergence is the infallible sign of the planet's rotation, and the invariably perpendicular radius-vector is the fixed point where the rotation begins and ends. If the rotation be made in the same direction as the orbital revolution, or from west to east, to the extent of one degree of circumference for each degree of the orbit, the planet will, in every complete orbital revolution, inevitably bring every point of its circumference in succession once to the radius-vector, and there present it directly to the centre; and from this undeniable fact it follows inevitably that if the same point of circumference remains continually in one with the radius-vector, and directed to the centre, that permanence of its relation to these members of the orbit, which are in themselves invariable in their relations to the planet's axis, must be, and *is*, the certain token of the planet's *non*-rotation.

But, as astronomers say, if the moon were *not to rotate* on her axis, she would carry every point of her circumference in turn past the radius-vector, backwards, or from east to west, and present them all in turn to the earth in or near the centre of her orbit, and, at the same time, keep all her parts perfectly parallel to a fixed straight line drawn across the plane of her orbit, and that permanent parallelism is a proof of her non-rotation. The moon would, indeed, if she were perfectly *free* in her movements, under the single condition of keeping her *axis* within the limits of a definite orbit, exhibit these phenomena, and preserve that perfect parallelism; but, *horresco referens*, that would most certainly be the effect of her rotation on her axis backwards, or in a direction *contrary* to that of her revolution, namely, from east to west, while she revolves from west to east. And to keep in countenance this paradox, it is assumed, not tacitly or inadvertently, but directly and expressly, that, in this case of retro-rotation, or so-called parallelism, it is the radius-vector, nay, the centre of the orbit itself, that moves in the opposite direction from the point *a* of the moon's circumference! But I have endeavoured to show already, that the radius-vector and the centre are perfectly incapable, by their very nature as such, of making the smallest change in their relations to the moving body; and that, whatever change takes place in the relations of the moving body's circumference, is, and must be, solely and exclusively the effect of the rotary movement of the planet, or satellite on its own axis. And the parallelism which has been assumed, and so triumphantly referred to as the token of non-rotation, would be occasioned by so very simple a process as this. When the planet turns round on its axis from east to west, the axis itself, while it preserves the same invariable relations to the radius-vector and the centre, is continually changing its place on the orbit, and of course its relations to every surrounding object, outside of the orbit, all round the compass; and, as the rotation is performed in precisely the same proportion as the orbital revolution, degree for degree, but in opposite directions, the result is *apparently*, though not *really*, to neutralize the rotation, and so to keep the parts of its mass always directed to the same points of the compass, always parallel to themselves, and to a fixed straight line drawn in the plane of the orbit.

A planet, moon, or other round body revolving in an orbit has a certain definite breadth or thickness, and the orbits or circles described by the different parts of its mass are consequently of different dimensions, proportional to their radial or diametric distances from the orbit's centre. The innermost point of circumference *a*, will describe an orbit *less* than the orbit described by the body's central axis *c*, by the amount of the measure of one circumference of the body, while *c* will, in its turn, describe an orbit also one circumference *less* than the orbit described by the outermost point of circumference *b*. Thus, when the body revolves without rotating, *b* will be found to travel the measure of *two* circumferences, and *c* *one* circumference, more than *a*. When, therefore, *a* diverges from the radius-vector to the left, and turns round about the axis from east to west, returning to the radius-vector from the right, it travels, by virtue of such rotation, just the measure of *one* circumference more than the length of its own proper orbit, and consequently through a measured space, or along a measured line, precisely equal to the dimensions of *c*'s orbit; and in no other way, and by no other means than rotation round *c*, could it accomplish such a feat. In the meantime, however, *b*, and the succeeding points of circumference, which all take the outmost place in their turn, have travelled the measure of one circumference more than *c*, and consequently more than *a* likewise; and that additional length of the outermost point's journey, has the effect of making both *a* and *c*, as well as *b* itself, preserve the parallelism referred to, while not the less *a*'s rotation on the axis is a *real* and *actual* event; there being the measure of *two* circumferences travelled by the outermost point to be met by only *one* circumference in excess on *a*'s part, and consequently there being that space available for the purpose of keeping *a* always in the same parallelism, and *apparently* without rotation; but such an *appearance* can deceive only those who *neglect* to inquire into, or who *wilfully overlook*, its cause. All the three points, *a*, *b*, and *c*, will be found in this case to have travelled precisely the same measured length of space in the same time, and *rotation on the axis c*, is the indispensable means of producing such an equality in the rate of movement. If, on the contrary, there be no rotation, the point *a* will remain constantly in one with the radius, and *a*, *c*, and *b* will revolve in three concentric orbits, at different distances, and with different velocities, proportional to these distances, *b* travelling one circumference more than *c*, and *two* circumferences more than *a*, while *c* will also travel *one* circumference more than *a*, and *one* less than *b*. And this is the *real* and very simple and obvious cause of the parallelism. The point *b*, as a part of the circumference, rotates backwards as well as *a*, and by so retro-rotating loses the value of *one* circumference in its orbital journey, while *a* gains one; but *b*'s place on the outermost circle or orbit is continually supplied by the other points of circumference in succession, including *a* itself in its turn, at the 180th degree of the revolution; and the combined gain of these points altogether is just that *one* measure of circumference which serves to maintain the parallelism.

It must be obvious, from the least consideration of the circumstances, that the outermost point of the revolving body's circumference, whether it be a fixed point, as in the moon's case, or a continually changing point as in the case of the earth, must necessarily describe a larger orbit than the innermost point; so that, whether the body rotate on its axis or not, the outermost half of its mass will move through space with a greater velocity than the innermost half, or, which is the same thing, than the central axis, in the proportion of one circumference more than the length of the orbit which the axis describes; and the effect of this surplus movement is, that, if the body rotate on its axis *once* during the orbital revolution, it will present every point of its circumference in succession *twice* to every fixed object outside, *one* of these presentations being the effect of rotation on the axis, and *the other* being the effect of the superior velocity of the outer half of the mass, that superior velocity itself being directly the effect of the constrained movement of the axis, in obedience to the centripetal force which keeps it in its orbit. When the body revolves without rotating, the same point of circumference will remain continually in one with the radius, on the same side of the axis, and on the *inside* of the orbit, without the slightest variation in its relation either to these members of the circle, or to its centre, but will present every point of its circumference in succession *once* to every fixed object outside, and that by virtue of the superior velocity of the outer half of the mass, which proceeds during the revolution, without affecting the rotation on the axis. Let the diagram (Fig. 1) represent the moon as at each of her quarters; and suppose that she, without quitting these points, turns round on her axis 90° at each. At north, the point *a* will be facing the earth; but, by turning round 90°, there *a* will get into the line of the orbit, 90° from the radius-vector as at W. At W., *a* will turn away other 90°, and will then be distant from its first position by the whole length of the moon's diameter, as shown at S. At S., *a* will again turn round the third quarter of 90°, and get into the position shown at E. At E., *a* will turn round its fourth quarter of 90°, and get into the position shown at N. In these quarter points *a* could get into the several positions it occupies in no other way than by the moon's retro-rotating, or turning round on her axis backwards, in the direction from east to west; and if this retro-rotation were to happen 360 times, instead of only four, as in the diagram, why should not a turning away of her circumference to the extent of one degree of the circle of rotation be allowed to happen at each of these 360 points, as well as a turning away to the extent of 90° at each of the cardinal quarter points, and with the same result, namely, *a* remaining always looking south?

With respect to a revolving body, all the results of her orbital revolution, and self-rotation during that revolution, would be most precisely the same; if, instead of revolving, she were to stand still, and the orbit, supposed a solid ring or disk, to revolve or turn round on its own centre, till every point of its circumference in succession were past the moon's axis or centre. Suppose this revolution to be made from east to west, being the direction contrary to that which the moon takes in her usual progress; that the moon herself were at the same time to remain constantly at the point N (Fig. 1) during the orbit's revolution, and to keep her point *a* constantly fixed on the radius looking to the earth; the result would be that the orbit would complete its revolution without producing the smallest change in the moon's relations to the orbit itself, to the radius-vector, and to the earth at the inner end of the radius, in other words, without causing or requiring her to turn round on her axis. But precisely the same result would follow the moon's own revolution in her orbit; the same face always looking to the earth, in one with the radius, and 90° distant on either side from the line of motion; what should make rotation necessary in this case, namely, to produce results that can be shown so distinctly and undeniably to be produced without it? Again, suppose the moon to remain constantly at N, and, while the orbit revolves from east to west, to turn round on her own axis also from east to west exactly once during the orbit's progress past her. Well, when the orbit's point W reaches north, the moon's own point *a* will have turned round to *b*; when the orbit's point S reaches N, *a* will have turned round to *c*; when the orbit's point E. reaches N, *a* will have turned round to *d*; and when the orbit's point N returns to the moon on the other side, *a* will have returned to its first position in one with the radius. But, the effect of this revolution of the orbit is that *a*, by turning round on the moon's axis, occupies at the moment at which each quarter point of the orbit reaches the moon, always the very position relatively to the orbit and the radius-vector that it would occupy, were the orbit to stand still and the moon to revolve, with her face always looking south, which astronomers say, would be the result of revolving in her orbit without turning round on her axis. Just lift the diagram, and turn it round in the proper direction, and this will be seen at once. It is, indeed, perfectly undeniable, and confutes beyond the possibility of redemption the ridiculous blunder that has so long disfigured the most perfect of the sciences.

To say, therefore, that the moon's presentation of the same point of circumference constantly to the earth, is the consequence of her rotation, implies nothing less absurd than an *orbital revolution of the earth itself*, round about the moon's axis, at the very time the moon is rotating about the relatively fixed earth! For, if the moon *rotate* with the same face constantly to the earth, the earth, to keep her in countenance, must inevitably rotate also, and in the same direction, round about the moon's axis, as if she, (the earth,) were only a part of the moon's circumference, or an outlying member attached by an inflexible radius. It might be possible for the two bodies to revolve, like the binary stars, about one common axis situate between them, and such, I believe, is the fact; but for them to revolve about each other's axis, at one and the same time, is perfectly impossible; yet nothing less than this perfect impossibility is required to support the received doctrine of the moon's rotation. But, perfectly impossible as it is, no less great men than Galileo and Laplace have both jumped at once to this conclusion, as the only discoverable means of getting out of the dilemma occasioned by the *parallelism* which we have now discussed and accounted for. The perfect impossibility of the earth's and the moon's simultaneous revolutions about each other may be demonstrated by the experimenter's taking a rod or stick, with a ball on each end, and making the two balls revolve about each other, *if he can.* One of them may be made to revolve round about the other at rest, and both may be made to revolve simultaneously about an axis between them at some intermediate part of the stick; but to make them revolve simultaneously round each other, he will soon find to be perfectly impossible. Yet, in the face of this utter impossibility, the great Laplace scruples not to say, that a person in the centre of the moon, supposed transparent, and revolving about the earth without rotating on his, or her, own axis, that is to say, preserving the parallelism we have spoken of, would *see the earth and her visual ray revolve about him*, that is, round about the moon's axis, in the space of a month. But, true it is, that the earth, considered exclusively as the centre of the moon's orbit, supposed for the present a perfect circle, moves not a hair's breadth from her central position, and, from that immovability of the earth, it follows certainly and inevitably, that any change of relation between the earth and the parts of the moon's circumference must be the effect solely and exclusively of a movement of that circumference away from the earth round about the moon's axis.

When a globe, disk, or wheel makes a forward movement, and changes place, turning round on its axis at the same time, no part of its rotating circumference ever *falls behind* the perpendicular line that connects its axis with the road, rail, or track along which it moves. All the parts alike move forward, and it is the perpendicular radius accompanying the axis, that moves still forward, past them, *leaving them behind* in the spot they occupy for the moment, and in which they are kept back, not by virtue of a rotatory power inherent in the revolving body, but by a foreign power or force acting from without, and which only yields them gradually to the impulse of the forward-moving power, to take their turn in the forward movement. It is obvious, therefore, that if the same point of the moving body's circumference remain always in one with the perpendicular radius, (and it makes no difference whether the line of motion be a straight line or a circle, for in both cases alike the radius-vector, (in a perfect circle at least,) is invariably perpendicular to the line of motion, and always as far forward as the axis itself,) that point so remaining in one with the radius will always be on the same side of the body and its axis, and there will not, as there can not, be any rotation. The extrinsic force that retards the successive points of the circumference has no effect upon the perpendicular radius-vector; it never keeps it back, and consequently it does not keep back or retard that point of the circumference, whatever it may be, that is always in one with the radius. That point, so far from rotating backwards, is constantly moving forwards; and this is true in all circumstances; the infallible, and I may say, the only infallible sign of rotation being, that the several points of the wheel's circumference in succession pass the radius-vector, and cross the line of motion itself, 90° distant from the radius on either side. And this happens, when a body revolving in an orbit presents the same point of its circumference invariably to the same fixed point of the plane in which it moves; for in such a case that seemingly invariable point of circumference will be found to diverge from the radius on one side and return to it on the other, to *cross the orbit* of the axis twice, once in going out and once in returning to the inside, and assuming in its progress every various degree of distance from the centre of its orbit, in proportion to the amount of progression. This ever varying change of its relation to the orbit's centre is the result of a forward movement on the part of the revolving body; but neither does the axis of the revolving body ever change its distance from the orbit's centre, nor does the centre itself of the orbit ever change its place at all; consequently, it must be the point of the revolving body's circumference that changes *its* place, seeing that its relation to the two invariable centres is constantly changing. The axis does not revolve about it, so as to get between it and the centre of the orbit; the centre of the orbit does not revolve about the axis of the planet, so as to fall behind the changing point of circumference; what remains therefore but to say, that it is the changing point itself that revolves about the axis of its own body, while that axis revolves about the immovable centre of its orbit? When, at the second quarter of an orbital revolution, the seemingly invariable point of circumference is outside of the orbit, farther distant from the orbit's centre, by a whole diameter of its own body than it was at starting, how could it possibly have got into that position, unless it had performed half an orbital revolution, or half a rotation, round the moving axis? It is no doubt true that it has not rotated under the impulse of an intrinsic rotatory power acting upon it in a circular direction. It (*i. e.* the point of circumference in question,) in common with every other point of the circumference, and generally of the planet's mass, is carried into the respective and successive positions which it and they occupy by virtue of the centrifugal force that carries the body through the circumference of its orbit; and it is the extrinsic centripetal force, acting from the centre of the orbit, that pulls continually the revolving body's axis, and makes it deflect from the straight line it would follow, and, by so pulling it, causes it continually to *leave behind* the most advanced points of its circumference, whatever these may be for the time; and the ultimate result is, that every point of circumference in succession *is left behind in its turn by the axis*, till the point which started in one with the radius-vector is again in one with it, after having performed a complete circuit round the axis, just as if, all the time, its motion forward had been retarded by friction or other obstacle acting upon it from the outside of the orbit, and pulling or keeping the circumference back, while the axis moved forward; and this circular movement of the circumference is most unquestionably *rotation*, or turning round about the axis, though it have been caused by the movement of the axis itself, that movement of the axis being in a direction contrary to the natural movement of the parts of its mass under the influence of the forward-moving power.

All that I have now said may be demonstrated mechanically by the simplest means, and in the simplest manner, no more costly or more complicated apparatus being required than a common bottle-cork, with a long-tailed string tied round its middle, and a pin with which to fix it to the centre of a circle, and to serve as an axis or pivot of revolution and rotation. But I shall use an instrument of somewhat more complexity, though still within the reach of every one who wishes to try the experiment. But first let us consider the diagram fig. 2, where E and M are two circles of equal dimensions. Suppose E to remain fixed and immovable while M revolves about it from west to east. As M revolves, its several points, 2, 3, 4, 5, 6, 7, 8, 1, will present themselves successively in that order to E's corresponding points; but, in doing so, M will necessarily turn round *once* on its axis, and all the points in succession will pass the radius-vector, or straight line that connects the centres of the two circles, and of course revolves with M round E's centre point. Let M revolve a second time, with the same point of its circumference No. 1, always in contact with E.'s circumference, and in one with the radius-vector. It will revolve without giving the smallest token of rotation, yet say astronomers, M in this case has turned once round on its own axis, though that has been rendered impossible by its adherence to the radius-vector. In the first revolution also it turned *once*, and no more than *once*, why should the *one* invisible and imperceptible rotation which they ascribe to the moon, produce such different results from those of the visible, real, and undeniable single rotation of M, as shown in the diagram, and of every other body similarly situated, or revolving in an orbit? Let M revolve a third time, turning its points from east to west, or what is the same thing, preserving the perfect *parallelism* of all its parts to the two sides of the paper, it will present its points successively to E's points in the reverse order, 8, 7, 6, 5, 4, 3, 2, 1, to E's, 1, 2, 3, 4, 5, 6, 7, 8, and each of them in its turn will pass the radius-vector *once*. In this case it is said to revolve in its orbit, *without turning round on its axis;* yet has it shown every requisite token of rotation, and carried its several points in succession to an always increasing angular distance from the orbit's centre, till each of them has become the outermost point of its circumference, and from that extreme distance of 180°, as shown by its position below E, returned *by the other side of the axis* to its first position, just as if M. had turned round on its axis all at once, without moving forward in its orbit. How can these points get to the *outside* of the circumference, unless the body turn round on its axis; or, what is the same thing, unless each of the points describe an orbit of rotation round M's axis, which remains invariably at the same distance from E's centre? Let this question be answered if it can consistently with the theory of lunar rotation.

The real rotation of the revolving body, in the case of so-called parallelism, may be shown by this experiment. Make the disk M of pasteboard or stiff paper, mark it as in the diagram, and fix it by a pin through its centre to the outer end of a material vinculum or radius-vector, which shall revolve by its other end round E's centre. Above the disk M, also on the pin, place a smaller disk, just large enough to show distinctly the same marked points of circumference; and then place the radius with its two disks on the line NS. In revolving, both of the disks on the pin would keep their places, with their points 1, 1, on the radius all round; but while the smaller disk, which is virtually nothing more than M's axis itself expanded, for the sake of distinct perception of the changes it might undergo, keeps steadily with 1 to the radius, make M turn round on the pin continually, precisely in proportion to the velocity of the orbital revolution of its axis, degree for degree, and minute for minute, and it will preserve its *parallelism* throughout the revolution; but that it has not the less *rotated* is unquestionable; for the experimenter's own fingers have pushed it round, carrying its successive points of circumference continually past the radius-vector, and away from E's centre, to the extent of 90° or a quarter rotation, in each quadrant of the orbit; while, on the other hand, the small disk certainly has not turned round upon its axis, the pin; the pin has not turned round in its place on the radius; and the radius has not *turned back* at E's centre, or changed in the slightest degree its proper perpendicular relations as the shortest straight line between the centres of E and M; or, in other words, it has not rotated or turned round about on M's axis, any more than E's centre itself, with which such a rotation or a change of position in respect of any point fixed or moveable on the circumference, is impossible. What remains then but to admit the fact, that the so-called parallelism is produced by M's retro-rotation from east to west, while its axis revolves from west to east, with all the parts and molecules of the mass moving along with the same degree of velocity under the influence of the centrifugal power acting upon them through the axis? On the other hand, were both the disks alike to keep the same points 1, 1, on the radius, it follows certainly from the same premises that neither of them rotates on the pin axis, but that the mass of each simply revolves from west to east with different degrees of velocity proportional to the diametric or radial distances of its particles from the centre of E, just as if they were integrant particles of E's own mass, turning round about their (E's) own axis. The radius-vector is, in all cases and circumstances without exception, virtually an integrant or integral part of the orbit and its centre, and can no more change its relation to that centre by revolving or rotating about the axis of a planet or satellite than the orbit's centre itself can. Such a revolution or rotation of the orbit's centre is absolutely impossible; and for the radius-vector it is equally so. The axis, moreover, of a planet or satellite, is, like the radius-vector, only an imaginary line incapable of any movement independent of the mass of which it forms a part. Therefore, it cannot be said in any case that the axis itself rotates while the body does not. In the case, however, of a common wheel, with a separate axle, that might be said; but I have shown already that it would be said without ground.

This same diagram will serve very well to show the nature and the cause of that *parallelism* which has been mistaken for the effect and sign of non-rotation. Each of the disks has a diameter of one inch, and a circumference of three inches, or thereabouts; consequently the orbit described round E's centre by the innermost point of M's diametric breadth *a*, measures *three* inches also; while the orbits described by M's central axis and the outermost point of her diametric breadth *b*, measure respectively *six* inches and *nine*. These several orbits are described by the revolving body equally in all the three cases of revolution just spoken of, showing most plainly that, whether the revolving body turn round on its axis or not, it is not in the least degree affected in respect of rotation or non-rotation, by the great difference of dimensions that exists between the orbits of its outermost and innermost points of circumference, whether these be always the same points, or a series of points changing continually. It might appear, indeed, that if the outermost point travel so much faster than the innermost point, it would push the latter round the axis; but this is prevented by the axis itself moving forward always in line with the two points *a* and *b*, and the effect of that forward movement of the axis is that it (the axis) and *b* virtually revolve about *a*, instead of *a*'s partaking with *b* in a rotation round the axis. Strictly speaking, they, along with *a*, revolve in one line round about the centre of the orbit, just as if they were all three points in the lineal extension of the radius-vector. This is really the case when the body revolves without rotating; but, when it does rotate, the points of circumference change places continually, while the radius-vector alone, or the points in its length, describe the two orbits corresponding to the outermost and innermost points of circumference, the axis itself remaining always on the radius, in line between them. If the three points *a*, *b*, and *c* (axis) were to move along with equal degrees of velocity, it is perfectly true that at the end of the first quadrant of *c*'s, or the middle orbit, *a* and *b* would be perpendicularly in line with it, or parallel to the meridian NS; and a rotation backwards (from west to east) would bring *a* back again to the radius. But, in fact, the moon does not move in this manner; the same point of her circumference remains constantly in one with the radius, always on its own orbit, at the same distance from the centre; and as the three points of her mass really do revolve under this condition, the result is that, when *a* finishes its quadrant, *c* and *b* would be 90° behind it, unless a rotation or forward movement to that extent *bring* them into their proper places upon the radius, in line with *a*. This is effected gradually by their moving faster than *a*, in proportion to the greater dimensions of their respective orbits, so that they remain in line with *a* upon the radius, all the way round, without disturbing *a*'s or their own relations to the radius and the centre; or, in other words, without the body's rotating on its axis. The cause of the moon's not rotating backwards, from east to west, or preserving her *parallelism*, as she naturally would, were she to revolve with her circumference *free*, and her axis alone subjected to the centripetal force, we shall consider by and by.

Take a stick or rod of any kind, and attach it by one end to a fixed point on a floor or a table, and then to one side of its outer end attach a disk of any size or substance by a nail or pin stuck through its centre. Make the rod revolve from west to east round the point to which it is attached, and at the same time keep the disk continually turning round on its axis. Turn it round and round, backwards or forwards, its relations to the radius and the centre will remain unaffected by the revolution of its axis in its orbit, though the relation of that axis to every object outside of the orbit will be continually changing; and this happens because the revolution of the axis carries forward the whole mass simultaneously with, and in the line of, the radius-vector, without changing its relations to the points of its own circumference. If the same point remain always in one with the radius, it will, of course, never change its relation to the axis. If the whole circumference pass the radius once in an orbital revolution, it will change its relations to the axis once also, and *only once*; but, if that change and passing of the radius once be made in the direction from east to west, the parts of the mass will preserve their so-called parallelism, by virtue of the forward motion of the axis in the contrary direction, which affects the relation of the circumference to extra-orbital objects, but not at all to the axis itself, or to the radius-vector and the centre. The same thing will appear still more clearly, if possible, by a variation of this experiment, Make a large disk, and fix it to a table or floor by a pin through its centre, to serve as an axis of revolution. Near the outer edge of its circumference attach a small disk by a thread through its centre, to serve as its axis. Draw a straight line between the two axes to represent the radius-vector, and then make the large disk revolve on its axis, and it will be seen at once that, in whatever direction it may be turned, the small disk will remain as unaffected by the movement as if it were an integral part of the mass, and that the relations of the several points of its circumference to the axis and the radius are affected by its own movements *only*, round its own axis, quite independently of the movements of the large disk to which it is attached. The lid of a small pill-box will answer nicely for the small disk, if a thread be attached to one point of its circumference, having its other end fixed to the centre of the larger disk, and the large disk be made to revolve with the *parallelism* of the small disk preserved, the thread will be drawn all round the small disk, showing incontestably that disk's rotation on its axis.

Upon the face of a wall, or other flat perpendicular surface, draw a circle one foot in diameter, and divide it into quadrants by two diametric lines crossing at right angles in the centre. Then take a stick, staff, or rod, one foot long, with four square sides, and attach it by a hinge or other ligature to a fixed pivot in the centre of the orbit. At the middle of the rod, which of course will be the distance of the orbit from its centre, fix another pivot, (on the rod itself, not on the wall,) and on that pivot hang a string with an open loop, having at its other end a plummet which shall keep it stretched as far as the centre of the circle, when the rod is placed perpendicularly with its whole length above that centre. In other words, the string, including both loop and plummet, must be of precisely the length of half the rod, or six inches. Mark the quadrants as N, W, S, E; N and S being at the opposite ends, N at the upper, and S at the lower end of the circle's perpendicular diameter, while E. will be at the right hand end, and W at the left hand end of the cross diameter. Then attach the rod to the centre of the circle on the line N S with its own middle or pivot *c* on the orbit at N; and on the latter pivot hang the looped string and plummet. The lower half of the rod, while so attached to the centre of the circle, will, of course, be identical, or virtually the same, with the mathematical radius-vector of the pivot *c* to which the plummet is hung. When thus prepared, set the machine in motion, making the rod revolve round the central pivot of the circle, from west to east, that is to say, from right to left at starting, so continuing always forward in the same direction. As the rod revolves, the plummet will be seen diverging from it to the left, in the same proportion, degree for degree, moving away from the centre of the circle, just as its own pivot *c* moves away from N; but keeping the stretched string always parallel to the perpendicular diameter NS, and to the two cross walls to the right and left of the room where the experiment is made; and when the rod completes its first quadrant and reaches W, the plummet will have also described a quadrant for itself of the same dimensions, will have crossed the orbit to the outside, and will have reached a point at the angular distance of 90° further from the centre of the orbit than the place it occupied in contact with the centre just before starting, and at right angles with the rod and its axis, showing, in my judgment, a rotation of 90° round that axis. In the second quadrant, the plummet will again describe a quadrant for itself, and will reach a point still further distant from the primary centre by another 90°, or, altogether, 180°, directly outside of its pivot. But, how could it get there, unless it had performed a rotary movement of 180° round that pivot? It has, unquestionably, by its regular movement under the law of gravity, described a semicircle of that angular extent, and of the same real dimensions as the orbit of its revolving axis; and the change of their relations to the primary centre cannot be ascribed to the revolving axis, for that axis has kept at precisely the same distance from the primary centre, and with its radius-vector always subtending the same rectangle with the orbit, during the whole passage of 180°; and, as it is the plummet's real and visible movement that has brought it into that situation, that movement must be resolved into its elements, as partly progressive along with its pivot, and partly rotary about that pivot, the effect being precisely the same, as if the string and the plummet had lain upon the rod during its passage of the quadrants, and then been set at liberty, when it would have run through the angular distance of 180° at one sweep, and something more, only settling at the 180th° point, after oscillating till tired. Through the next two quadrants, this process will be repeated, in the reverse order, and when the rod returns to its first position, the plummet will again be in one with the centre of the orbit. The diagram, (Fig. 3,) shows the relation of the orbits described by the two moving bodies, the continuous circle being the orbit described by the pivot or middle point of the rod, and the dotted circle being the orbit described by the plummet. To prove further the reality of the plummet's revolution round the pivot *c*, take a flat cylindrical disk, thick enough to carry a cord on its edge, fix it by its centre on the revolving pivot of the rod, mark its quadrants, N, S, W, E, and to the point S, in one with the line NS of the primary circle, attach the plummet by its string. Then make the rod revolve, as before, from west to east; the plummet will be seen again describing an orbit of its own, partly within and partly without the primary orbit; but, the disk being fixed to the rod, and not turning on the pivot, the plummet's cord will gradually wind itself round the disk's edge, and when the rod has completed its revolution, the plummet will again be in one with the diametric line NS, but its cord will be round the whole circumference of the disk, a thing which could not possibly happen unless the plummet's rotatory revolution carried it round that edge.

At each quarter point of the circle on the wall, draw a strong and distinctly marked cross, and mark the points of each that are nearest the centre, *a, a, a, a.* It is obvious, from the nature of the case, that they cannot turn round on their axes; but if it so happen that the rod AB moves successively past them all in the course of the orbital revolution of its own axis, while its true or mathematical radius-vector preserves invariably the same relation to each of the four crosses, and to each of the four straight lines that connect these crosses with the centre, is it not equally obvious that that radius-vector moves forward along the orbit continually, and altogether in the same direction with all its points, without turning round on the axis of the revolving rod any more than the fixed crosses themselves turn round on their axes? They have not rotated on their axes; how comes it then that the radius-vector, rotating as alleged round the moving axis, is always in one with the same unmoved arm of each of them? Again, across the rod AB, at its pivot or axis, place a cross bar of say three inches length, and attach the ends of this cross bar to the two ends of the rod by wires or tight strings, forming altogether two triangles, on the opposite sides of a common base, and having the vertices respectively at the opposite ends of the rod, as in Fig. 3. It is obvious from mere inspection of the diagram that if the two ends of the base change their relation to the orbit, by the one going outwards and the other going inwards, that change implies, and can only be the result of, *a rotation on the axis.* But, if the two ends of the base rotate, the rod, from whose points they are 90° distant on either side, must rotate also, whether that rotation be from west to east, or from east to west. In both cases alike it must diverge from the radius-vector, which can never forsake the centre, or change its relation of perpendicularity to the orbit; but, in diverging and rotating from east to west, *contrary* to the direction of the orbital revolution of its axis, the rod will necessarily preserve its much vaunted *parallelism*, that parallelism which deceived Galileo, and has deluded all his followers for two centuries. On the other hand, if the diverging of the cross-stick from the line of motion necessarily implies rotation, the remaining of that cross-stick on the line of motion equally implies *non*-rotation; and, when it does remain so, the rod AB itself will remain always in one with the radius-vector, which never does, and never can, turn round about the revolving axis.

The plummet being removed, and a long loose cord attached to the same point of the cylindrical disk, and having its other end fixed to the primary centre of the circle, make the rod revolve, and it will complete its revolution without affecting the cord, without drawing it round the cylinder, showing unequivocally that there has been *no* rotation there. Now, detach the rod from the primary centre, and make it revolve, preserving its parallelism with the line NS, so that its inner point A will now describe exactly the same orbit for itself as that described by the plummet. When it completes its revolution in this parallel fashion, strange to say, though it has revolved, as astronomers allege and profess to believe, without turning round on its own axis, it will nevertheless have drawn the cord round about the fixed cylinder which has been revolving with it, also without rotating on its axis, as alleged. But how can the cord get round the cylinder if the cylinder has not rotated on its own axis? And if the diameter of the cylinder were to be made as large as the rod itself, then the cord would be drawn round both the points of the revolving rod, notwithstanding its pretended avoidance of rotation by preserving its parallelism. And that the movement which has occasioned the drawing of the cord round the cylinder has been exclusively on the part of the rod itself, will be evident from the fact that the cord's movement begins at the end attached to the moving rod, and is affected through its whole length, and at its central end only, when the cylinder's rotation has drawn it all away from the centre; and, moreover, by the fact that when the rod was not loose, and consequently did not move away from the centre, the cord remained entirely unaffected by the pivot's revolution, showing that there was no motion in the opposite direction, on the part of the primary centre and the radius-vector; and if there was no such opposite motion in that case, there could be just as little when the rod was loosed, and it and the cylinder left at liberty to turn away from the centre.

To meet such arguments and illustrations as these, astronomers and mechanicians have nothing better to offer than the following notable proposition. "Let AB (Fig. 4) represent an inflexible rod of no weight supported by a hinge at the point A, in the centre of a circle, in such a manner that it may turn or revolve freely round that centre. When it so revolves, its motion may be divided into two; one progressive, by which its centre of gravity C changes its relative position to other bodies, and the other rotatory round that centre, *by which its parts change their positions with regard to one another. If it make a complete revo-*

lution round the circle by its point A (attached to the centre by a hinge) *its parts in the same time will make a revolution round the centre of gravity* C." The first and most obvious remark upon this notable proposition is, that the latter part of it, all that is in italics, is wholly untrue, and most directly contrary to the truth; and I cannot comprehend how any man in his right mind should ever have been able to persuade himself that it is true, unless, indeed, it were on Tertullian's principle of believing a thing *because it is impossible!* When a rod so situated revolves round the centre of a circle, not only "its centre of gravity C," *but also all the other particles of its mass,* "change their positions relatively to other bodies;" while, on the contrary, were it to rotate on its axis C, whether that rotation be visible or invisible, "its parts would *not* change their positions with regard to one another." They would, on the contrary, preserve invariably the same relations to each other, to the orbit's centre, and to their own axis or centre of gravity, but would change their relations continually to everything else. This seems to me perfectly self-evident. It must follow, at least, from what I have shown already; and the only *reason* astronomers can give for maintaining such a proposition is, that, if it were not true, their doctrine of the moon's rotation would not be true either. The doctrine and the proposition are thus perfectly consistent with each other, but their truth is just as problematical, and, not to mince the matter, just as *false* as the ancient doctrine which placed the earth in the centre of the universe, and made the sun, moon, and stars, the whole host of heaven, revolve about it. The proposition above quoted has not even such *appearance* of truth; for it is just as directly contrary to *appearance* as to *fact;* and no man in his senses would ever have believed it, had he not first persuaded himself, on other grounds, that it must be true, in spite of appearances. In short, the proposition is not an induction from observed facts, but only a deduction from an erroneous hypothesis, contradicted by facts.

The latter part of the proposition, "if the rod makes a complete revolution round the circle by its point A (*attached to the centre by a hinge*), its parts in the same time will make a revolution round the centre of gravity C," is just as utterly untrue and contrary to fact as all the rest. Take two rods of equal length, each attached by a hinge to the centre of the same circle, but on opposite sides, so as to form together one diameter of the circle, or, what would do as well or better, take a single rod of double the length of the rod AB, and fixed by its middle upon a pivot in the centre of a circle or orbit, whose circumference shall be described by the middle part of each half of the rod. Call these two centres of gravity C and *c*, and the two halves AB, with its axis C, and DE with the axis *c*. These are virtually two distinct rods, and as such are radii of the circle to whose centre they are attached, and whatever is true respecting one of them must be equally true respecting the other. If, therefore, the rod AB rotate on its axis C during the orbital revolution, the rod DE must, by parity of reasoning, rotate on its own axis *c*. At the same time both rods together rotate, as radii of the circle, round the centre of the circle; so that each of them, during a complete orbital revolution of their respective axes, performs three distinct revolutions round about no less than three distinct centres *all at once:* AB turns round, 1*st*, the common centre; 2*d*, its own axis C; and, 3*d*, DE's axis *c;* while DE also turns round, 1*st*, the common centre; 2*d*, it own axis *c;* and, 3*d*, AB's axis C. The same thing must happen with every pair of opposite spokes in every wheel: each spoke is the radius of a circle, attached to the centre like AB; and if AB rotate on its own centre of gravity at the circumference of the circle, so must each spoke, placed as it is in precisely the same circumstances, revolve round its own centre of gravity, and, with the help of its opposite neighbour, perform *three* circular revolutions, round three different axes, where no human eye can see, and no human understanding can discover the possibility of more than one, namely, that round the axle of the wheel, the common centre of all the radii. Again, if AB turn round on its own centre C, each half of AB divided by C will also, in each rotation of the rod round C, turn round on its own centre of gravity, and so on, as far as matter is divisible; so that in every case of a body's revolving on an axis, we should have an infinite multitude of wheels within wheels, all revolving and rotating at once; but which will be all swept away at once, like the Ptolemaic cycles and epicycles, by means of the simple truth, that in the case of the rod AB and of all bodies similarly situate, as the diameters and radii of wheels, moons, and planets, there is and can be only *one* revolution round *one* centre, namely the centre of the orbit, *so long as the same point of the rod, the same end of the diameter, or the same point of circumference of the revolving body remains in one with its radius-vector and the centre of the circle.*

But, say mechanicians and astronomers, if the rod AB were not to rotate on its axis, its point A would move away from the centre, and all its parts, in length, would remain perfectly parallel to the straight line NS of our first experiment on the wall. To be sure it would, but the nature, the cause, and the result of such a movement and such a parallelism have been abundantly shown already. Such a movement of the point A from the centre would be an act of rotation round its own axis C. Moreover, such a rod as AB has no inherent power of rotation to *bring* its points *back* to the radius-vector; therefore the "hinge" of the proposition is used as a substitute, and the effect of that hinge is to hold A fast, and *keep him from rotating* round about C, as he naturally would were he *free* to obey the impulse of the centrifugal moving power. It is indeed an absurdity to talk of *bringing back* what never goes away. The correct expression in the moon's case would be, that her inherent power of turning round on her axis, if she really possess such a power, in a direction contrary to that of the impulse given to her parts by the centrifugal power, *neutralizes* that influence so far as to *keep* the same point of her circumference continually in one with the radius-vector, and so to make her revolve in her orbit continually, *without turning round on her axis*, to a greater extent than that indicated by her libration in longitude, which is a real oscillation on her axis, occasioned by the ellipticity of her orbit, and her varying velocity in the different parts of that orbit.

The same thing may be proved by an experiment on a great scale, performing continually between earth and heaven. The earth's centre is not the centre of the moon's orbit; but the two bodies together have a common centre of gravity situate at a certain distance between their axes, and round that centre of gravity both axes revolve. The distance between the earth's proper centre or axis and that centre of gravity is the measure of the radius-vector which connects the earth's axis with that centre of revolution, and is just a prolongation of the moon's radius-vector, as if the two radii-vectores together, however different in length, formed but one inflexible rod revolving on a pivot at the centre of the system. But, if it be true that the moon's radius-vector revolves about the moon's axis, and revolve about that axis it must, of the point of the moon's circumference that is always in one with it, so revolve, as alleged, then the earth's radius-vector must also revolve round the earth's axis; and both radii forming virtually *one* inflexible rod, must, like the double rod, AB, DE, simultaneously perform three distinct revolutions; 1, round the common centre of gravity; 2, round the earth's axis; 3, round the moon's axis; *all at once*, which is perfectly impossible.

The preceding argument and illustrations respecting the rod AB apply precisely to the case of a globe revolving in close contact with the centre of its orbit; the diameter of such a globe being represented by the rod AB; and whatever is true or false respecting the rotation and revolution of that rod, is equally true or false respecting the revolution of the globe whose diameter it represents. The effects and changes produced by the revolution and rotation in immediate contact with the centre of its orbit are not in any respect different in kind from the effects and changes that are or would be produced by the revolution and rotation of a globe at the far end of a radius-vector, of any conceivable length, even to the orbits of Herschel and Leverrier, or beyond them; for the radius-vector is the instrument and accompaniment of the revolution, virtually a part of the orbit itself, representing the corresponding point of the centre's own circumference, at the point of the orbit where the globe may happen to be for the time, and the index of the globe's rotation, or turning round on its own axis, every point of the globe's circumference inevitably passing the radius-vector once in every complete rotation. Metaphysically considered, indeed, the centre of an orbit is an imaginary point, without length, breadth, and thickness; but considered physically, it is a certain portion of space, with a certain definite circumference, however small, divisible into any number of parts or degrees like the orbit itself; so that the orbit is virtually the centre itself expanded to those limits, as, on the other hand, the centre is virtually the orbit itself contracted to the smallest conceivable limits; consequently, whatever could be truly predicated concerning any point of the orbit, or concerning a body moving along the orbit, could be just as truly predicated concerning the same or a similar event at any point of the centre itself.

The principle, and the fact of retro-rotation, may be beautifully and perfectly illustrated by the movements of a magnetic needle in a compass-box. Fix the box, containing the needle, to the outer end of a rod or radius-vector that shall describe a circle round its other end fixed in the centre, in such a way that the two ends of the needle shall be pointing directly to the north and south points of the card fixed immovably in the bottom of the box. Then make the radius revolve from west to east, carrying of course the box along with it. As it revolves, the card will be seen to carry its point N, in the same direction away to the left of the needle's north point, and so on round the whole circumference, every one of its points in succession passing the needle, which will seem to stand still, and remain invariably pointing in the same direction, or maintaining an invariable *parallelism*, as if it were facing the sun or other stationary spectator outside of the orbit. Now, in this case, one of three things happens,—1*st*, the box *fixed*, though it be to the radius, turns round on its own axis; or, 2*d*, the radius itself, carrying the box along with it, turns round on its own centre of gravity or the pivot in the box; or, 3*d*, the needle, which is *movable* on its pivot, turns round on that pivot, while the box and the radius revolve without rotating. To prove which of them it is that rotates, just *fix* the needle in line with the radius, in the direction NS, so that it shall not move on its pivot; then make the rod revolve, and box and needle alike will remain in the same invariable relation to the radius-vector and the centre of the orbit. But, at the first quarter, or at any other intermediate point short of the half-circumference, set the needle at liberty and it will fly back to its natural position, directly north and south, while the box and the radius will remain as before, in the same undisturbed relation to the orbit's centre. The cause of the needle's rotation is well known, and sufficient to account for the phenomenon; while there is no apparent or probable cause of rotation in either the box or the radius. It is, therefore, the needle that rotates, and neither the box nor the radius; yet the effect of that rotation is to keep the needle in the same direction of *parallelism*, the very result which, in the moon's case, astronomers would ascribe to her *non*-rotation.

A ship sailing round the world is placed in circumstances precisely similar to those of the moon. It describes a circular pathway on the earth's surface, with just as much freedom of movement as the moon herself possesses in her orbit. If it were to move under the influence of the centrifugal or tangential force alone, represented in its case by the wind or steam that drives it along, it would move straight forward, and continually lift every point of its circumference in succession from the water, just as the moon, if equally free, as astronomers assume that she is, would turn her points successively away from the earth. But the moon, it is said, possesses a power of rotating on her axis in a contrary direction, and that power "*brings*" the outward turning point continually "*back*" to the radius. It is not easy, indeed, to conceive what can be meant by "bringing back" what not only never goes away, but on the contrary *keeps* invariably the same position. To speak, therefore, more correctly, we should say that the alleged power, if it exist, *keeps* the same point of the moon's circumference continually in one with the radius-vector, and prevents its ever moving away at all, so as to require "*bringing back.*" It just neutralizes the natural tendency to turn her circumference outwards, which the moon would have if she were free. But the ship, it will be admitted, does not possess such a power to bring its prow continually back to the water, and so to keep its keel continually directed to the centre of the earth. By what means then does it exhibit, in precisely similar circumstances, the same phenomenon as the moon, if it want what is considered indispensable to the production of that phenomenon in the lunar globe, that ship sailing in the sky? Why does not the ship present its topmast and keel alternately, and indeed every point of its perpendicular circumference in succession, to the earth's centre, which is the centre of the orbit in which it revolves? It must either do this or bring the same point continually back to its radius-vector by virtue of some power intrinsic or extrinsic, like that ascribed to the moon. But the only power known or ever supposed to act upon the ship in these circumstances, is the attraction of gravitation; and if that power be sufficient to keep all the ship's particles in the same relative places with respect to the earth's centre, why should it not be sufficient to account for the moon, in such precisely similar circumstances, exhibiting the same phenomenon, that of presenting the same face continually to the centre of her orbit? In assuming the existence of such a rotatory power as that ascribed to the moon, astronomers seem to be not only guilty of the common fallacy of *non causa pro causa;* but even to fly directly in the face of Newton's laws of philosophizing, which tell them that they ought never to assume more causes of natural events than are *true*, and sufficient to account for the phenomena; and that, of natural effects of the same kind, as in the case of the ship and the moon, the causes are the same. Would not that man be thought mad who should seriously affirm that the earth's centre, along with the radius-vector, revolves about the centre of gravity of every ship that sails round the world, nay of every ship, animal, locomotive engine, and cloud, that describes an arc on, or near to, her surface? Yet, this is just what astronomers do say when they affirm that the earth with the moon's radius-vector revolves about the moon's centre during each of her orbital revolutions. Think of the earth's centre performing a gyration, or thousands of millions of simultaneous gyrations or oscillations, round about all, and each individually, of the bodies that change place on her surface, *without ever forsaking its own proper place*—the centre! Yet, say astronomers, the earth herself revolves about the moon, without ever leaving the centre of the orbit! I think, after this, I may say with confidence that the earth and the radius revolve about the moon just as much as the earth's centre revolves about the round-the-world-sailing ship, and no more, that is to say, not at all; and that the moon, like the ship, revolves in her elliptic pathway, without turning round on her axis, any more than the ship turns round on its centre of gravity.

It is to the action of the centripetal force, or of the earth's attraction of gravitation, that I ascribe the fact of the moon's non-rotation. Under the influence of the centrifugal force alone, acting upon its whole mass, while the *central axis exclusively* was affected by the centripetal force, the moon would preserve the *parallelism* already so often mentioned; and astronomers seem all to assume that the moon's circumference, and the particles or molecules of her mass between that and her axis, are entirely free from, and unaffected by, the centripetal force of the earth's attraction; and it being thus *free*, an inherent or intrinsic power of self-rotation is required to *bring* the same point of the circumference continually *back to the radius-vector.* But if the doctrine of gravitation be true, then the earth's attraction acts, not merely upon the moon's axis to the effect of keeping her in her orbit, but also upon every point of her circumference, and every molecule of her mass, with a force proportional to the radial distance of those particles from the earth. The earth's attraction is consequently strongest upon the nearest parts of the moon's circumference, and its effect is to neutralize the forward moving impulse of the centrifugal power on those particles, and hold them fast, while the axis and the outward parts move forward in obedience to the centrifugal impetus. The general result is that the parts of the moon's mass revolve in their respective concentric orbits with different degrees of velocity proportional to the dimensions of those orbits, without rotating on her axis, which actually describes a larger orbit, or moves faster forward than the inner point that is said to rotate round it. Astronomers are indeed perfectly aware of this real influence of the earth's attraction, though they have all agreed to overlook it; for they say, to use the words of Mrs Sommerville, varied a little from Laplace, "the lunar spheroid has three principal axes of different lengths at right angles to each other. The one directed towards the earth is the greatest. The *attraction of the earth*, as if it had drawn out that part of the moon's equator, *constantly* BRINGS *the greatest axis*, and consequently the same hemisphere, *towards us.*" If, instead of *brings,* they were to say *keeps*, this quotation would, in my opinion, express the true state of the case; for, certainly the point remaining in one with the radius-vector never falls back a hair's breadth; it moves constantly and continually forward with the radius-vector, in line with the axis itself, and never changes its relation either to the axis of the moon, or to any thing else within the orbit of that axis, except to a small extent occasioned by the eccentricity of the orbit, the earth's position in one of the foci, and the varying velocity of her progressive movement. To that extent the moon vibrates or oscillates on her axis, but she certainly never completes a rotation.

This oscillation or vibration, very properly called the moon's *libration* in longitude, is said by astronomers to be the effect or result of the *uniformity of her rotation*, and the *irregularity* of her orbital movements; for "the motion of revolution is sometimes slower, sometimes more rapid. The apparent vibration, therefore, which it occasions cannot always exactly counterbalance the actual rotation, which remains constantly the same; and these two effects will surpass each other by turns." But, the moon's so-called "uniform rotation" being no rotation at all, some other cause must be sought for the libration than the one assigned; and it seems to be this.

"The Moon," says Sir Isaac Newton, "is revolved about its axis by a motion *most equable* in respect of the fixed stars, viz., in 27 days, 7 hours, 43 minutes, that is in the space of a sidereal month; so that this diurnal motion is equal to the mean motion of the moon in its orbit: upon which account the same face of the moon always respects the centre about which this mean motion is performed, that is, the exterior focus of the moon's orbit nearly; and hence arises a deflection of the moon's face from the earth, sometimes towards the east, and other times towards the west, according to the position of the focus which it respects: and this deflection is equal to the equation of the moon's orbit, or to the difference betwixt its mean and true motions, and this is the moon's libration in longitude." The moon, at apogee, directly faces the earth; but, when she leaves that point to move along the west side of her orbit, if she were to move under the influence of the centrifugal force alone, that force would make her inner points of circumference turn gradually outwards, in the direction from east to west preserving their parallelism; but, as she proceeds, the elliptic orbit becomes gradually flatter, and that increasing flatness proportionally diminishes her tendency to retro-rotate, or parallelize, in as much as the direction of the centrifugal impetus gradually approaches the line of the orbit described by her axis, and consequently carries forward all the particles of her mass in lines always becoming more nearly concentric to or parallel with that orbit. At the same time she is getting continually nearer the earth, and that approach directly increases the earth's power of attraction on the nearest parts of her circumference, enabling it to hold them faster. She is also continually increasing her orbital velocity, and the immediate effect of that increase is to leave the attracted portions of her body more rapidly behind, while the less attracted axis and outward parts roll forward. None of the parts ever move backward; for, if they did, that would imply two motions in the same body, in contrary directions, at the same time, which is impossible. They are simply left behind in the forward positions which they had previously gained. By the combined operation of these causes the result mentioned by Sir Isaac, that the moon's nose or point *a*, in one with the radius-vector at apogee, instead of being turned 90° away from it, at the end of the first quadrant of the orbit, is just where it was, not, indeed, in one with the radius-vector, but directly in one with the centre of mean motion, several degrees to the eastward or right hand of the radius, which of course is directed to the eccentric earth. After passing this point, the flexure of the orbit begins again to increase, which increases the tendency to retro-rotate; the velocity is still continually increasing also, which increases the opposite tendency of the axis to leave the attracted points of circumference behind; and the result of the antagonism is to bring the point *a* again to the radius-vector at perigee. From perigee backwards to apogee, the same process is gone through in the reverse order, along the other side of the orbit, so that, when the revolution is completed, *a* is again in one with the radius. But the variations which the moon exhibits are the effects of real *rotation*, or turning round on her axis, to the extent of the libration; and the whole process is a real *libration* or balancing on the axis, and not merely an optical deception, as some astronomers call it, though it may be true enough that the earth's eccentricity with respect to the orbit makes the libration appear larger than it would be, under the effect of the antagonist forces only. If the moon were *not* to rotate at all, the same point *a* would remain continually in one with the radius-vector. The radius-vector, however, being nothing in itself but an imaginary straight line drawn between the earth's axis and the moon's, the divergence of *a* from that line indicates a real movement of the moon's circumference past it, in the direction of the libration for the time; and the return of the point *a* to the radius equally indicates an oscillation of the moon on her axis in that direction, for the radius-vector itself never turns backwards on the moon's axis. Its motion is always forward only, along with the axis. These movements, however, are subject to the perturbing influence which act upon the moon, to the effect of changing continually the form and extent of her orbit; and so far the libration will vary considerably from what it would be in a regular and steady elliptical orbit. In a perfect orbit or circle there would be no libration.

Paradoxical as it may seem, I believe the same causes that *prevent* the moon's rotation actually *produce the rotation of the earth on her axis*, and such different effects of the same cause are to be accounted for by the earth's very great degree of velocity compared with that of the moon. She flies along her orbit *thirty times* faster than the moon in hers; and this amazingly greater velocity makes her axis and outer mass leave the intra-orbital parts of the mass for the time being so much more rapidly behind than the moon's comparatively slow movement makes her axis leave her inner circumference behind, that the difference of effect, so far from contradicting the hypothesis, seems to afford a strong confirmation of it. Various hypotheses have been suggested to acount for the rotation of the earth, and the moon, and the other planets; but the one that seems to have been received with most favour, and to have been considered as the most probable, is that of John Bernouilli, who "has shown on mechanical principles that one impulse would produce both simultaneous revolution in the orbit and rotation on the axis, and even calculated the precise point at which it must have been applied, so as to accord with the existing motions and velocities" of the earth and the moon.[*]

But Bernouilli's hypothesis proceeds on the assumption, common to him with other mathematicians and astronomers, that the planets and their satellites move about *freely* in space, under the single condition of revolving in certain definite orbits, a condition which affects their centres only, leaving their circumferences and all the other particles of the mass perfectly free. But if there be any truth at all in the Newtonian doctrine of gravitation, or in the common doctrine of the terrestrial tides, all the particles of the circumference and the mass alike are affected by the mutual attractions of the different revolving bodies, those particles that are nearest the external attraction being more, and those farthest off being less affected than the centre itself, which nevertheless, consistently with Bernouilli's hypothesis, must be considered to be the only part affected at all, the attracting power passing by and through the intervening particles without giving token of its presence. Moreover, the rotatory effect of the oblique impulse which he postulates could be permanent only in the case of a body revolving freely in space, that is to say, with only its mathematical centre attracted to the centre of the orbit. But the earth is not such a body, and there is not within our reach any projectile whatever, that can be made to move along an orbit or a curve unaffected by the earth's attraction of gravitation acting upon every particle of its mass from without, and directing or controlling its movements; and it appears to me that without such an external cause of rotation as gravity supplies, the rotating tendency produced by the oblique impulse would cease as soon as the movement was communicated to the whole mass, which would then fly forward with all its particles in one direction only; for, as already remarked, they cannot move backwards. All that is wanted is a power to keep them back, and that is more likely to be found *without* than *within* the moving body. It is a negative power not very likely to be found in any revolving body. Though, therefore, a stone, or ball, or quoit, thrown from the hand with a force acting obliquely on its centre of gravity, turns round on that centre, yet every calculation respecting the cause and consequences of its movements, that leaves out of account the earth's varying degrees of attraction on the different parts of the mass and their effect upon its movements, must be erroneous. To me, therefore, it seems unquestionable that, even if the earth had been originally set a-going by some such oblique impulse as Bernouilli supposes, the rotation would have stopt as soon as the whole mass was set in motion, and that it would at all events have become instantly subject to, and been modified and controlled by, the sun's attraction of gravitation, and even stopt at once if it had been in its direction incompatible with the action of the solar centripetal force. In the case supposed, however, its direction would be perfectly consistent with the action of that force, which, so far from hindering the rotation, would rather aid it; but which, at the same time, would be perfectly sufficient of itself to produce that rotation, without the primary oblique impulse supposed by Bernouilli. Suppose the earth to have been travelling straight forward in space at her present mean orbital velocity, but without turning round on her axis, when she came within the influence of the sun's attraction, that power would not merely grasp her centre, but would affect her whole mass with various degrees of force, proportional to their distances from the sun, and those attracted most would naturally feel the influence of the attraction in the *retardation* of their forward movement, while the parts farthest off would continue to fly forward with comparative freedom, and the greater the forward velocity of the mass in its orbital revolution, the more rapidly would the attracted parts be left behind, to take their turn in the rotation as soon as relieved from the sun's grasp by the lever power of the forward particles acting through the centre of the revolving body. In the rotation of a progressing body, as formerly observed, no part of it ever moves or can move *backward;* all the particles alike move forward, and only forward, and their retardation is in all cases to be ascribed *more probably* to friction, or attraction, or other hindrance from without, which are all, in their respective degrees and qualities, more or less *sufficient causes,* than to any intrinsic power of rotation, from whatever cause originating; and more especially when such a supposed hidden cause is not indispensably essential to the production of the visible effect. Bernouilli's hypothesis, therefore, is clearly at variance with Newton's rule of philosophizing, which requires us never to assume more causes of natural events than are *true,* and *sufficient* to account for the phenomena. It cannot be called *true* so long as it is improbable, and it cannot be called *sufficient,* inasmuch as it ascribes two kinds of movement in contrary directions in the same body, to the same cause, namely, the action of the centrifugal power, and leaves quite out of consideration the antagonist influence of the centrifugal force upon the circumference of the revolving mass. It is the action of the centrifugal force that makes a body, revolving in an orbit with its circumference unaffected by a primary's attraction, turn round on its axis from east to west, or preserve its parallelism; and if the function of making it rotate in the contrary direction, from west to east, were to be assigned, as it is by Bernouilli's hypothesis, to the same centrifugal power, that power would be brought into the rather awkward predicament of acting as its own antagonist, in producing two contrary movements at the same time in the same body, and thereby neutralizing its own efforts. The hypothesis, therefore, may be fairly rejected as insufficient and untrue.

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[*] "Jean Bernouilli, dans un memoire de dynamique, ou il considere les centres spontanés de rotation, fait voir qu'une force de projection appliqué non pas au centre de la terre, mais un peu plus loin du soleil, et cela de $\frac{1}{150}$ du rayon, donneroit à la terre supposée ronde et homogeneè, deux mouvemens assez conformes a ceux que l'on observe ; pour la lune $\frac{1}{150}$. Si l'impulsion primitive eut été appliqué a des plus grandes distances de chaque centre, le mouvement de rotation seroit plus rapide. *Encyclopedie, Art. Rotation.*

If the earth were affected exclusively by the sun's attraction of gravitation, her rotation, I believe, might be easily, simply, and sufficiently accounted for in the manner I have proposed; but she does not move alone in her orbit, and, with such a companion as the moon, the problem becomes somewhat more complicated, though scarcely more difficult; for the moon's attraction acts in the same direction as the sun's, and helps to turn the body round; and not merely *helps*, but probably she is the principal agent, her power, from her nearness, upon the earth's circumference being much greater than the sun's. To this view of the case there may be one objection started, namely, that the liquid portion of the earth's surface moves westward, contrary to the rotation, which would not happen if the moon herself were the cause of the rotation. But the earth's rotation is not the effect of the moon's action only; it is produced by the combined operation of its own centrifugal power, the centripetal power of the sun's attraction, and the moon's power; and the effect of the combined causes is to make it rotate faster than the moon revolves, so that the solid parts of the rotating mass continue to move forward, while the liquid parts, more easily affected by, and consequently more obedient to, the moon's power, are left behind, to a certain extent, by the solid parts, which slip away from below them. The waters of the ocean do not really move backwards, or in a direction contrary to that of the earth's rotation; they only, owing to their liquidity and greater susceptibility of external affections, rotate more slowly than the solid parts; but still they do participate in the general rotatory movement, passing the moon every day, as well as the dry land. "What," says Punch, "is the cause of the earth's rotation? Probably, *because one good turn deserves another*." This jocular piece of philosophy is not so far from the truth as might be imagined; for *one good turn* not only deserves, but really tends to produce another; and the uniformity and regularity of the earth's rotations are just the result of a long series of "good turns," every one of which has tended to help forward its successors.

The sun's rotation may be accounted for in the same way. All the planets and all their satellites act upon him as well as he acts upon them; their combined power altogether is not sufficient to move him from his place, but it may be quite sufficient to pull him round on his axis. The planets all revolve in the same direction; and their pulling power acting, as it has acted for countless ages, may have resulted at last in a mean movement of rotation, just like that of a fly wheel, which moves with regular velocity notwithstanding the irregular application of the moving power. The planets may be considered as so many men or horses pulling by so many ropes at the circumference of a monstrous wheel, which, when once set fairly in motion, continues to move of itself, with a velocity proportional to, as well as produced by, the various efforts of the combined powers. And, perhaps, if the sun himself be only the satellite of some mightier centre, his rotation will have to be ascribed in part to that also, as well as the earth's to the action of the sun.

The error respecting the moon's alleged rotation seems to have occasioned, or served at least to confirm another error respecting the *measure of rotation*, and the *equation of time*. "The length of the astronomical mean day, which is divided into 24 hours, is determined by the interval which elapses between two consecutive passages of the sun over the meridian of the same place, supposing this apparent motion of the sun to be performed with an uniform velocity. But it is necessary to observe, that our earth *does not occupy quite* 24 *hours in its rotation*, on account that in the same time, which it employs in revolving round its axis, it advances in its orbit towards the east about a degree in space, corresponding to 4 minutes, or more exactly to 3 minutes 56 seconds of time. Hence it follows, that the interval between two passages of a fixed star over the same meridian, *which measures the true time of the earth's rotation*, or of the sidereal day, is only 23 hours, 56 minutes, 4 seconds. The *sidereal day* is the period in which the earth makes *one complete revolution on her axis, and is measured by the interval between two successive transits of any fixed star over the same meridian.*"

The slightest consideration should suffice to show the absurdity of such a notion. Suppose a body to revolve in a perfectly circular orbit; and its own circumference to be of exactly the same dimensions as the length of one degree of the orbit described by its innermost point of circumference. As already remarked, every revolving body has a certain definite breadth or thickness, which causes its particles to revolve in different orbits proportional to their radial distance from the orbit's centre; and, if the orbit described by the body's innermost point of circumference, *a*, be exactly equal to 360 measures of the body's circumference, it is obvious that the return of *a* to the radius-vector will be the measure and indicator of each rotation; yet, in every such rotation, any given star will appear on the same meridian sooner than at the preceding transit, and these daily gainings, or losses rather, of the star will amount to one sidereal day in the course of the year. From this it follows most obviously, that the transit of the star is not, and cannot be, the measure of rotation, inasmuch as the measure it gives is 4 minutes of time, or 1° of space, shorter than the measure of the revolving body's real circumference, which, as already supposed, measures exactly one degree of the inner verge of the orbit. The centre of the orbit never changes its place in the slightest degree; the radius-vector is equally invariable in its relation to the revolving body; it never *falls back* a hair's-breadth; its whole length moves constantly forward, and forward only, turning round on the centre, and the centre only, and not at all round about the revolving body's axis; consequently, its relation to the orbit's centre, and to the body's axis, is precisely the same at every degree of the orbit's circumference. When, therefore, *a*, in rotating, turns away from the radius, the radius moves forward just *one measure* of the body's circumference, and meets *a* again at the end of the first degree of the orbit; and the same thing is repeated in every one of the 360° of which the orbit consists. If the body were to stand at any point of its orbit, and turn just once round on its axis, it is most obvious that the return of the same point *a* to the radius-vector would be unanimously acknowledged to be the completion of just *one* rotation; but, what should make a difference, when the rotation is performed, not all at once at the same point, but only gradually, minute for minute, as it were, during the body's progress in the orbit? No *reasonable* answer but the true one can be given to this question, and that true answer is, that there is not, and cannot be any difference; and that in a perfect orbit, with a revolving body describing equal areas in equal times, which, in that case, would be with perfectly uniform velocity, the radius-vector indicating the transit of the sun within the orbit, and not the transit of the star outside, would be the true measure of rotation.

The phenomenal relations of the heavens and the earth are the same, whether we ascribe them to the earth's orbital movement round the circuit of the heavens, from west to east, or to the circumvolution of the heavenly sphere itself, in the opposite direction, or from east to west, round the earth at rest in the centre of the universe. Suppose then the earth to remain at a fixed point of space, while the sphere of the heavens turns round about it, and to turn round on its own axis 360 times during the period of the revolution of the stars in the ecliptic; the sun all the time remaining in the centre of the orbit, in one with N outside, or, as it were, straight south of N, north. Commencing its revolution at N (fig. 5,) and moving from east to west, or to the right hand, while the earth at the same time turns round on its axis from west to east, the star will continue to move always further and further away from N, to the extent of one degree, during each of the earth's rotations. But the earth is fixed in the same point of space, and, so long as it is so, its rotation can only be complete when the same point of its circumference *a* returns to the fixed point N; and the opposite point *b*, 180° distant, again faces the sun, and is in one with the radius-vector, or straight line that connects N and S. In making this return, *a* will every day, or in every turn, be in one with the off-moving star four minutes sooner than it reaches N; consequently, its rotation will then be incomplete to the extent of one degree of its own circumference. On the second day, *a* will point to the star 8 minutes before it reaches N; on the third day, 12 minutes; and so on through the whole series of the earth's 360 rotations or days, during which it will have seen the star 361 times, the sidereal day being 1-360th part shorter than the solar day, which measures the earth's complete rotation. It is, however, perfectly obvious and undeniable that in this case the difference is occasioned by the star's moving away from N, and not by N's moving away from the star; consequently, the star, by so moving away from N to an always increasing angular distance, presents itself to the earth's point *a* four minutes of time sooner in each rotation, or every day. It is, on the other hand, just as perfectly obvious and undeniable that the rotation is incomplete till *a* return to N; and that being so, either N itself or a straight line drawn between it and the sun, must and can be the only true index of the rotations. Now, just reverse the process, and let the star stand still at N, while the earth revolves from west to east, rotating as before, and carrying of course its radius-vector NS along with it, which radius will revolve round S, as its pivot of rotation. When the earth completes her first quarter, the star, remaining at N, will be in precisely the same relation of place to the earth, as it was, when it (the star) had reached its own first quarter of rotation, while the earth was remaining at N; and so on throughout the whole revolution of the earth in her orbit, which is precisely the converse of that of the stars, their mutual relations being precisely the same in both cases. From this it follows inevitably that it is the sun's transit, indicated by the passage of the point *a* or *b* across the radius-vector, and not the transit of any star over any given meridian of the earth's surface, that is the true measure of the earth's rotations, or at least that would be so, were the earth's orbit a perfect circle, along which she moved with uniform velocity.

Suppose the earth, instead of rotating 365 or 360 times in each orbital revolution, were to rotate only *once*. By rotating *once*, however, be it observed, I do not mean that she should keep the same point of her circumference constantly on the radius-vector, looking to the sun; on the contrary I really mean to say what the word expresses, that she should indeed turn once round about on her axis *like a wheel*, carrying every point of her circumference past the radius-vector, and across the orbit. Of course, as already remarked, and as admitted by all astronomers, the results would be precisely the same, whether the star revolve round about the heavens from east to west while the earth stands still at N, or the earth move round in the contrary direction, from west to east, while the star stands still. Let the earth, therefore, for the present be considered as standing still at N, and let the star revolve to the right, or from east to west. By the time the star reaches the first degree to the right of N, the earth will have turned on her axis one degree in the opposite direction, making together a difference of *two* degrees; and this being repeated at each degree, the star will make its transit over the same meridian which it left behind at N, when it has only gone round *half its circuit* of the heavens, and the earth has turned only *half round* on its axis. After a similar process, during the other half of the star's revolution and the earth's rotation, the star will make a second transit over the same meridian, or in other words, the star and that meridian will again be in one at N, after completing their respective circuits. But it must be perfectly obvious, that in this process the earth has turned round on its axis *only once*, and measured its complete rotation by carrying every point of its circumference, without exception or shortcoming, past the radius-vector and the sun; and consequently that the radius-vector and the sun's transit are the sure and only measures and indications of complete rotation, while on the contrary, the star and its sidereal day falls short of that complete measure by no less than a half rotation in this particular case. If the earth were to rotate twice, the star would have three transits; and, in short, for every number of the earth's complete rotations in a year, the star will make one transit more, which is just the proportional value of its shortcomings with respect to the rotations. Let the star now stand still at N, while the earth revolves from west to east, turning round on her axis *once*, that is, at the rate of one degree of her circumference for one degree of her orbit, and precisely the same results will follow, without the slightest difference in the phenomena. As before, the star will fall so far short as to make *two* transits during the revolution; or *one* transit more than any number of complete rotations the earth may make. And by their own admission of precisely similar results in the two cases, astronomers are precluded from objecting to this conclusion of the argument, which entirely confutes their doctrine of the sidereal day's being the true measure of the earth's complete rotation.

It is true, indeed, that the outermost point of the earth's circumference describes a much larger orbit than the innermost point, and from this fact astronomers infer that the point *a*, after really completing one rotation, will take four minutes more to make up the difference. But, it has been shown already, that this difference in the dimensions of the orbits has no effect whatever upon the *rotation* of the moving body; and that it only causes the axis to move forward more rapidly, and change its place in relation to other bodies, while, with respect to itself, all the parts of the body's internal mass and external circumference remain in precisely the same relative positions, whether they rotate or not. That is to say, if the body do not rotate, that relation of the parts to the axis remains absolutely invariable, however much the axis itself may change its place in the orbit, and its relation to other bodies; if, on the contrary, the body rotate, the rotating parts change their places just precisely to the same amount in the same time, as they would, if the axis were to stand still; the whole effect of the more rapid motion of the outer circumference being *not* to make the circumference rotate more rapidly round the axis, but to make the axis itself move forward so much faster than the inner point of circumference as virtually to describe a circle round that very point, instead of that point revolving round the axis. And this is fully confirmed by what has just been said in illustration of the converse movements of the heavens and the earth.

The variation in the length of the solar day is ascribed to the difference of the sun's velocity in different parts of his orbit; but the only way in which such different degrees of velocity could affect the length of the day, would be by making him, or more correctly, the earth turn more rapidly, or more slowly, as the case may be, round on her axis; and thereby to make her rotations not *uniform*, or of precisely the same length. In opposition, however, to this legitimate inference from the alleged cause, astronomers say that the earth's rotation is precisely uniform, and is measured by the sidereal day, which, says Mr Barlow, "is the most uniform of all astronomical periods, neither observation nor theory having yet detected in it the least variation." By "uniform" is meant that the successive transits of the same star over the same meridian of the earth's surface happen after always precisely equal intervals of time; but it must be obvious from the slightest consideration, that if the solar days are not uniform, neither can the sidereal days be so; for the meridians over which the sun's and the star's transits are made are the same, or at least corresponding meridians of the same surface of the same revolving body, and consequently liable to the same variations and disturbances in all respects. If, therefore, the "uniformity" of the sidereal day be an observed and positively ascertained fact, some other cause of the variations of the solar day than those assigned by astronomers must be sought for; and that cause is most readily to be found in the *ellipticity* of the earth's orbit, and the sun's eccentric place within it. The direct result of these circumstances is that the earth's radius-vector almost constantly subtends a continually varying angle to the orbit, sometimes approaching it on one side and sometimes receding. As the earth revolves from her aphelion point, the radius-vector diverges from the perpendicular, and gradually approaches the west side of the orbit, forming an acute angle with its vertex to the north, and consequently meeting the earth's meridians proportionally sooner every day. Soon, however, it begins to recede, and open out from the orbit, and consequently to make the earth's meridians take a proportionally longer time to reach it in their rotation. The same process is repeated four times, in converse order however, during the whole orbital revolution, and sufficiently well accounts for the difference in respect of "uniformity" between the sidereal and the solar days; for the stars outside, in respect of relative position, are quite unaffected by the variations of the radius-vector. Having no means of making accurate observations for myself, I can only reason on the data supplied by others. Taking, therefore, the common tables for the equation of time, I find them to agree remarkably well with the *cause* which I have now supposed. From this it will follow that mean and apparent solar time will coincide or equally show true time, only at those points where the radius-vector is perpendicular to the orbit. In a perfect orbit or circle, the radius-vector would be invariably perpendicular to the orbit, and consequently would always be the true index and measure of the planet's rotations, while the star outside would fall short of that true measure by more than three minutes every day.

In a popular treatise on science I find it stated that "any meridian on the earth will revolve from a fixed star to that star again in exactly the same time as if the earth had only a diurnal motion, and remained always in the same part of its orbit."

Undoubtedly this would be true, *if* the earth were really to remain "always in the same part of its orbit;" but the case is completely altered when she moves in the same direction as she rotates to a constantly increasing angular distance away from the star, for then the star will be seen, or will make its transit proportionally sooner every day, and, consequently, when the rotation is so far incomplete. "But," our author continues, "with respect to the sun, as the earth advances almost a degree eastward in its orbit in the same time that it turns eastward round its axis, it must take more than a complete rotation before it can come into the same position with the sun that it had the day before." This would be perfectly true *if the sun moved away from his place* so much proportionally every day as to require an additional turning of the earth to overtake him; but *the sun stands positively still*, and the earth's meridians meet him precisely in the same *place* every day, though not always at precisely the same *time;* for his relation to the earth is invariably the same, excepting to the small extent occasioned by his eccentricity and the ellipticity of the earth's orbit, with which points, however, we have nothing to do at present, and it will answer our purpose equally well on both sides of the question, to consider the earth's orbit as a perfect circle with the sun in the centre. This relation of the sun to the earth's axis and to the radius-vector that connects them, is entirely unaffected by the earth's movement in her orbit, and, were the earth's orbit a perfect circle, the sun and the radius-vector would be the true measures of the complete rotation in respect of *time* as well as of *space.* But all this I have shown fully already. Would the wheel of a cart, which is always moving forward, be considered to have made a complete rotation, if the spoke that was perpendicular to the ground at one turn, should fall short of that perpendicularity at a point in advance of the former point of the ground by the exact measure of the wheel's circumference? Say the wheel's circumference measures 15 feet, and it were to roll along a road divided exactly into marked spaces of 15 feet each, would it not at each mark present constantly the same spoke or point of its circumference perpendicularly to the ground? Why then should not the earth do the same, when the marks of her travelling path are as exactly measured, and as distinctly marked by the sun, as any points on any road can be? The earth's orbit, considered as a perfect circle, is divided into 360 perfectly equal degrees, with which the sun is connected by an equal number of radii at the same precisely equal distances; his transits, therefore, must be the best, the surest, and, indeed, the indispensable signs of the complete rotation of a round body moving along the circle's circumference. Moreover, the extension of the sun's 360 radii beyond the earth's orbit to the concave celestial sphere, "the firmament of heaven," would divide the whole visible universe into 360 apparently equal parts; and were any globe or mass of matter whatever to revolve in any one of the infinite multitude of orbits that might be drawn round the sun in the centre, and to rotate on its axis 360 times during each complete orbital revolution, then most assuredly each of its rotations would be complete only when the same points of its circumference, 180° distant from each other, or diametrically opposite, respectively returned to their places on each of the protracted radii, with their connecting diameter in one with the radius for the time; the same point facing the sun always at the end of each rotation, and the same opposite point always facing the corresponding point marked on the face of heaven by the protracted radius. On the contrary, every star on the ecliptic would be passed in its turn, and, in consequence of the angular distance continually gained by the revolving body, would fall proportionally short of the true measure of the body's rotation, making 361 transits when there were only 360 rotations.

What seems to have confounded the ideas of the writer from whose book I am quoting, and of all other astronomers, is that the sun turns round on his own axis in 25 days 10 hours, but it requires 27 days and upwards to bring back the same point of his circumference to the earth's radius-vector; *because*, while he is turning round from east to west, the earth is advancing in her orbit *in the same direction*, and, consequently, is always gaining upon the sun, who remains in his place, and is obliged to take more than a complete turn before the same point of his circumference can overtake the advancing earth's radius-vector. But this has nothing whatever to do with the earth's rotation, it being a matter of perfect indifference what part of the sun's circumference may present itself to the earth's meridian. Whether he rotate or not, the result, so far as the earth and her noons are concerned, will be the same; for it is the radius-vector directed to the sun's centre, and not a particular point of his circumference, that marks the moment of transit. But, owing to the movement of the two bodies (sun and earth) in the same direction, the earth will see the sun's whole circumference *one time* less than the number of complete rotations which he makes in a year, his rotations *as a fixed body* being measured and indicated, not by the flitting earth's radius-vector, but by his presenting the same point of his circumference again to the same equally *fixed* point of the heavens; that is to say, for every fourteen complete rotations on his axis, the sun makes only thirteen transits of the earth's meridians, not through any fault of his own, but owing entirely to the earth's moving away from the index of his rotations. The sun and the stars are alike perfectly fixed and immovable in their relations, and these their mutual relations are never in the slightest degree affected by the earth's movements. The changes of place which they, and particularly the sun, *seem* to undergo, are *apparent* only, and not *real*, and are the result exclusively of the earth's own continual change of place, and therefore the indicator of the sun's rotations must be a point of the heavens as invariable, or as little variable in position as he is himself, provided always he turn round on his axis with a uniform velocity, which there seems to be no reason to doubt. But if the sun and the earth were to change places and conditions, the earth becoming the fixed centre of the orbit, and the sun revolving about her as a planet or satellite, the case would be entirely different, for then the fixed and invariable axis of the earth and the sun's equally invariable radius-vector would become the true measure and indication of the sun's rotations, while, on the contrary, the star or other fixed point of the heavens, by which they were previously measured and indicated, would fall continually short of the true measure in proportion to the angular distance to which the sun would be continually moving away from it; and what would happen with the sun in such a case does most certainly happen in the case of every planet, satellite, or other body whatsoever moving in an orbit; namely, that when the orbit is a perfect circle, the radius-vector connecting the body's axis with the centre of the circle will invariably subtend or form a right angle with the orbit, and the centre itself will remain invariably in the same place, and this invariableness of the centre and the radius must most entirely preclude the possibility of rotation on the part of a circumference which keeps the same point continually in one with them. To my own judgment this is so perfectly evident, that I can never cease to wonder how any body could ever have doubted it, or gone so far wrong as to maintain the contrary received opinion.

An error of the like kind prevails respecting the true measure and indication of the moon's revolutions round the earth, as I find it stated in the following extract:—“The moon *performs its revolution round the earth* in 27 days, 7 hours, 43 minutes, 11 seconds, usually called a *periodical month.* It is to be observed, however, that the moon employs more than this time to return to the sun after each conjunction. The cause of this difference is, that the earth, and consequently its satellite the moon, advances in the ecliptic, while the moon is describing her orbit; so that before the moon comes into the same position relatively to the sun, 2 days and about 5 hours elapse beyond the time required for completing a revolution round the earth. The whole time occupied in returning to the sun is 29 days, 12 hours, 44 minutes, 28 seconds. This interval of time is called a synodical month, or lunar month. It commences from the moment when the moon is directly between the sun and the earth, in which position the moon is said to be in *conjunction*."

After what I have said already, this passage will require little comment. It is positively untrue, in so far as it asserts or implies that the earth's advance in the ecliptic occasions such a change in her relation to the sun, as to require an addition of nearly two days and a half to bring the moon round to the sun, after she has *completed* her revolution round the earth. The earth's relation to the sun is *invariable*, (except, as already so often mentioned, to the small extent occasioned by the ellipticity of her orbit). She finds him *in the same place* positively every day and every month, while, on the contrary, it is the star, to which our author most erroneously ascribes this invariableness, that experiences the change of relative position occasioned by the earth's progressive movement in the ecliptic. It seems to me the plainest dictate of common sense, as well as of right reason, that if I were to place a dozen of posts round a circle on the ground, to represent the stars, and one in the centre to represent the sun, and then to walk round the circle *within* the line of posts, I should pass every one of them in succession, and change my angular position with respect to each of them at every step of my progress, while, with respect to the solar post in the centre, the relation of the perpendicular axis of my own body to that post would be invariable, always at precisely the same distance, and with precisely the same right angle subtended on each side by my radius-vector with my orbit; and that, in consequence, if I were, at each of the twelve posts, to turn once round on my own axis, commencing with the solar post and the radius-vector in line with my nose, my rotation surely would not be complete till I found myself return to the same position, with my face direct to the sun; while, on the contrary, the post which I had last left behind, and the corresponding post 180° distant, would be seen to have changed their places to an angular extent corresponding to their positions in the circle, and pass my nose and back proportionally sooner than the completion of my rotation. And if, instead of making the rotation all at once at each post, I were to turn myself in the same direction gradually and proportionally, degree for degree, during my progress from one post to another, the result would most certainly be precisely the same. Is it not therefore more reasonable to consider the invariable right angles of the radius-vector, rather than the continually changing angles subtended by the stars, as the true indications of the completion of the planet's or satellite's rotation? Now look to the diagram, fig. 6, which will answer equally well to illustrate the subject of the earth's rotations and the moon's revolutions; the line of stars representing the ecliptic or concave circle of the heavens; the continuous ring, the orbit of the earth; and the body in the centre, the sun or other primary, as the case may be. When the planet or satellite is in its place on the orbit at N, it is then in line with both the star N and the sun, and were it to turn round on its axis at that point, both the sun and the star would equally indicate the completion of the rotation, because they are both, as well as the straight line that connects them, at right angles with or perpendicular to the orbit. At the same time, the straight line, forming in part the planet's radius-vector, continued across the orbit, points directly to the star S, at the angular distance of 180° from the star N. Then move the planet to the octant point NW. The diametric line of the orbit running through the planet's axis and the sun will then point to two different stars, each at the angular distance of 45° from the opposite ends of the line NS; but the sun will remain *just where he was, in precisely the same place* in the centre of the orbit, at the same direct distance, and still subtending with his radius a perfect right angle at the planet's axis on the orbit. The same process repeated at each octant or at each degree of the circle will shew precisely the same results. At the 180 degree of revolution, the planet will be in precisely the same relation to the star S that it formerly was in to the star N; which of them is to measure the rotation now? They both alike measure it exactly, *because* they are once more in a rectangular position with respect to the axis on the orbit; and *because* they have fallen short of the true measure of rotation to the extent of 180°, or half the planet's circumference each; so that, with respect to the planet, they have virtually come into each other's places. Precisely the same results would follow were the planet to stand still at N, while the circle of the stars revolved, if that were possible, round the centre of their circle, and then they would shew unquestionably that it is only where the planet's protracted radius-vector touches the circle of the heavens that the star which may be for the time at or opposite to that extremity of the radius, can indicate the completion of the planet's rotation. If, while the planet stands at N, and rotates without revolving, will the star NE be the true measure of rotation? No, certainly. Then just as little can the star N be the true measure of rotation when the planet revolves to NW. Let the small circle which has hitherto been considered as representing the earth's circumference now be considered to represent the moon's orbit, with the earth in its centre, and every thing that has been said will apply to the moon's case, as exactly as if she were really, what she is virtually, a point of the earth's circumference. She does not lag behind the earth as the latter progresses; she is sometimes as far before the earth as behind, her distance being not very variable, and consequently her revolutions must necessarily have the same measure of completeness as the earth's rotations. Why, then, should we resist the conclusion to which all this leads, namely, that the received opinion of astronomers on the subject of the rotation of the planets and their satellites is erroneous? The only plausible answer I can anticipate is, that Galileo, and Newton, and La Place, have sanctioned that opinion, and their authority is better than mine. But is it better than right reason and common sense together?

In the treatise on "Astronomy" published in "*The Library of Useful Knowledge*," I read, "We find not only that the duration of a sidereal day is constant, but that during every part of it the rotation goes on uniformly; or, in other words, *that the heavens revolve round their axis continually with an uniform velocity*.—The interval between the successive appearances of the sun upon the meridian, or from noon to noon, is necessarily longer than that between those of a star; for as the motion of the heavens is from east to west, and the proper motion of the sun from west to east, the sun on each successive day, when the point of the heavens where he was at noon on the day before returns to the meridian is to the eastward of that point, and consequently to the eastward of the meridian; and he, therefore, only returns to the meridian after the rotation of the heavens has continued for some additional period, long enough to bring his new place to the meridian, &c. The solar day is longer than the sidereal day in consequence of the motion of the sun eastward in his orbit, &c." I have quoted these passages, not for the purpose of making any direct comment upon the "scientific truths" which they embody; for such a comment as they require has been already anticipated, or will be still introduced elsewhere. But how could an author who seriously believes in the truth of the Copernican system of the universe deliberately affirm, to use his own italics, "*that the heavens revolve round their axis continually with an uniform velocity*;" or that "the sun moves eastward in an orbit?" Such a way of speaking must inevitably puzzle and perplex the readers of his book, as much as it seems to have puzzled and perplexed himself; for his ideas on the subject of the sun's and the earth's movements must have been utterly confounded when he affirmed to be positively true in fact, what could only be true if the earth were really to be placed in the centre of the orbit, and the sun were to be a planet revolving round about her, namely, that the star outside, and not the sun inside, is the true indicator of the earth's rotations, and of the moon's revolutions. But, when the case is reversed; when the sun takes his proper place as the fixed centre of the orbit, and the earth becomes the planet in his stead, all that was *true* before, in respect of his rotation and revolution, becomes *false* now; and it is from overlooking the consequences of this change of circumstances, that the error I am combating seems to have originated; and to this confounding of ideas, this mistaking of things different for precisely the same, the long continued prevalence of so very palpable a set of errors is to be ascribed.

Having, therefore, now finished my own case, and brought forward such arguments, proofs, and illustrations, as seem sufficient in my judgment to establish the *truth*, or what I believe to be the truth, I shall now proceed to examine such of the principal arguments and illustrations usually brought forward in support of the received opinion, as I have not yet had occasion to notice. I might, indeed, soon make up a large volume with the materials that superabound upon this subject, but I must be content at present with such as seem to be of sufficient importance to deserve notice. With respect to the *principles* of the whole question, the *invariableness of relation* of a planet's or satellite's axis and radius-vector to the primary's centre, and the ascription of every change of relations between the moving body's circumference and its radius-vector, exclusively to the rotatory movement of the revolving body itself, I have never had the smallest doubt or wavering since I read and understood Galileo's illustration of the subject, which I shall now present to my readers.

In his *System of the World*, Dialogue iii. he says, "Naturally, *without any moving cause*, a body freely suspended and carried round the circumference of a circle, *immediately of itself acquires a rotation*, that carries it round its own centre in a direction contrary to the motion which carries it round the circle, and with such velocity that both of these motions of revolution and rotation are completed in precisely the same time. This may be admirably illustrated by the following experiment. Take in your hand a basin of water; and turn yourself round on your feet, and you will see the water begin to turn itself round in a direction contrary to that of the basin, and finish its rotation at the same time with your and the basin's revolution. But, if you consider the matter attentively, you will perceive that the effect produced is nothing real, but only a mere appearance; that, what seems to be a rotation on its own axis, is really no motion at all; and that the water remains immovable in respect of every thing stationary beyond the limits of its orbit; presenting the same side or hemicycle continually to the same side of the room, or field, where the experiment is made, or to the same part of the sky. If, on the other hand, it be compared with the basin and yourself, who are both moving, it will be observed to change its direction, and to rotate with a motion contrary to that of yourself and the basin; so that it may be more truly said that the basin and yourself gyrate round the immovable water, than that the water rotates within the basin." So says Galileo; and truly, had all his other experiments been followed by such inferences as this, he must have occupied one of the lowest places in the ranks of science, if any place at all, instead of one of the highest. He must have been labouring under some strange delusion, when he persuaded himself to believe that a body which was visibly rotating before his eyes, and making all the points of its circumference in succession pass its radius-vector, or the straight line between its own centre and the centre of its orbit, and continually changing their distance from that centre, all the way round, was nevertheless immovably at rest. He seems indeed to have escaped the blunder of some of his successors, and to have been quite well aware that there were only *two* possible ways in which he could see a body all round, namely, by going round it himself, or having it turned round about before him; and accordingly, when he inferred from his experiment that the water did not rotate, his only alternative was to suppose that he himself and the basin must have gone round about the water. But it seems not to have struck him that this was impossible without leaving his central position and crossing the orbit twice, once in going to the outside and again in returning. Some of the schoolmen of old were of opinion that angels could go from one place to another without passing through the intervening space, and not without some shew of reason, for angels were supposed to be purely spiritual beings, without relation to time and space. Less reason than even this had Galileo for his opinion, when he supposed it possible for a heavy material substance like his own body, occupying a fixed position in space, to perform a gyration round about a distant point or body, not only without going through the intervening space, and measuring out by his movement the line of gyration, but even without ever stirring from the spot he occupied; his whole process of circumgyration being just as purely spiritual and imaginary as the angels' transit, and virtually the same thing as a planet's performing an orbital revolution without going round about the space which its orbit incloses, or through a single one of the 360° of which its orbit consists. Homer, the prince of poets, is said "sometimes to snore as well as nod;" so may it be said of the great "Tuscan artist," *aliquando bonus dormitat Galilæus.*

Galileo would have discovered his error at once if, instead of water, he had put into the basin a quantity of meal, flour, sugar, sand, earth, gravel, or any other sluggish material, and made it revolve, as before. In this case the matter would lie still in the basin, presenting, like the basin itself, the same side of its circumference continually to the centre of the orbit. Why does it not rotate or *seem* to rotate like the water? If, as alleged, the water does not rotate, does the more sluggish material, with the basin that contains it, perform that operation, by virtue of a rotary power naturally inherent in them? These materials exhibit the result which astronomers ascribe to rotation, yet keep all the parts of their circumference in precisely the same relation to the radius-vector, and to the line of revolution described by the central axis of the mass; while the *non*-rotating water carries every part of its circumference in succession past the radius-vector, which may be represented by a stick laid across the basin, and the line of revolution, and through all the various degrees of distance from the centre of the orbit, between its inner and outer edges, as determined by the diametric breadth of the moving body. Every point of the water's circumference is seen successively diverging from the line of the centre's orbital revolution, and the sum of all these successive divergences amounts to a complete rotation, or turning round of the body on its own axis or centre. It is indeed impossible to account for the different results of the experiments when performed with water, from those produced by the using of other substances, but by admitting the real rotation of the water, whose liquidity enables its particles to slip past each other in obedience to the impulse of the moving power; and denying the rotation of the basin and its more sluggish contents, whose particles are kept in their places, not by an inherent rotatory power, but by their *want of power* to overcome the friction that would instantly ensue among themselves, as well as between them and the sides of the basin, were they to attempt to move round about their centre like the water. Their adhesion, cohesion, vis inertiæ, weight, or gravitation, is too great to be overcome by the impulse of the moving power, while the lighter particles of the liquid and unresisting water fly forwards at once and continually till they get, all in their turn, round about the axis.

The experiment may be varied by placing a stratum of gravel in the basin, and pouring the water above it. Then make the basin revolve, and the liquid water will be seen as usual, moving away round, while the sluggish gravel lies still. Which of the two, the movable water or the immovable gravel, is the more likely to rotate? The water unquestionably. Astronomers, however, think differently, and say, or at least to be consistent ought to say, that it is the gravel, which rotates on its own axis, and thereby "*brings* the same point of its circumference continually *back* to the radius," while the water keeps all its particles *parallel* to themselves. But the gravel certainly does not possess any inherent power of rotating in that direction, while the water possesses not only the power, but also, what the moon has not, the *freedom* to rotate in the opposite direction; and using its freedom, away it flies round and round about continually, the particles of its circumference, and generally of its whole mass, changing their place-relation to the axis and to the radius-vector, and to the orbit and its centre, at every instant of progression. Even when water is used by itself in the experiment, a portion of its particles adheres to the sides of the basin, and remains behind, while the rest of the mass moves round about. According to the astronomic doctrine, the reason of this is, that the particles so adhering, and lying still with the basin, *rotate* or turn round on the water's axis, in that direction, while the rest of the water stands still. "*Credat Judæus Apella; non ego.*"

Galileo says that a body suspended freely in space naturally without any moving cause acquires a rotation round its own centre. But a moving cause there must be, and the cause of the water's rotation is this. The different parts of the water's mass have different orbits of revolutions, and, if they were to keep to these orbits, there could be no rotation, and the same point would be constantly presented to the centre of the orbit; but, as they are all carried round their orbits with precisely the same degree of velocity, by virtue of continued impulse of the same single moving power, that point of the circumference that is inside of the orbit, and nearest the experimenter, can find space for its journey only by going outside of its orbit, and consequently turning itself and the whole mass round on its axis to the same extent, while the outer portion of the circumference as necessarily falls within its proper orbit, leaving the centre alone to follow the mean orbit of the mass. In this way the three points, namely, the outermost point of circumference, the innermost point, and the centre, travel precisely the same distance, in the same time, with the same velocity; thereby causing a rotation of the body round its own axis; or, what is the same thing, preserving the perpendicularity and parallelism of its particles, while the axis is continually changing its place. This may be further illustrated in the following manner.

A body revolving in an orbit has a certain definite breadth, whereby it happens that its outside, or the outer part of its circumference is farther distant than the inside or inner part of the circumference from the centre of the orbit. Consequently, in travelling round the orbit these outer parts have a longer space to run through, which they can only accomplish by means of a greater degree of velocity. But, if it were required that the outer and inner points of circumference, A and B should proceed with neither more nor less than *precisely the same degree of velocity* as the centre C, and yet complete their revolution in *pre-*

cisely the same period of time, how would they be able to perform so marvellous a feat? Suppose the eight circles (fig. 7) to represent a globe revolving in a orbit round E. Suppose the centre of the revolving globe to be precisely 240,000 miles from the centre E; and its own diameter to be just 2000 miles. Its centre, therefore, will describe an orbit of 1,440,000 miles in circumference; but the corresponding orbits of the points A and B, whichare each 1000 miles from C, will be respectively 1,434,000 and 1,446,000 miles. To get through these orbits, *in the same time*, A would have to travel with a less, and B with a greater velocity than C; or, if they did not, it would happen that when C completed its revolution, A would be 6000 miles in advance, and B 6000 behind it; but cohesion of the parts prevents them ever getting farther away from C than 1000 miles; and, therefore, they must proceed in some other way than by orbits concentric with C's. In each octant of the supposed case, the difference in the lengths of the three octants is 750 miles; A's octant being so much shorter than C's, and C's so much shorter than B's. Suppose their revolution to be effected by means of a force acting in the direction of the centre C, or in such other way as that all the three points shall travel with precisely the same degree of velocity. When the revolving mass finishes its first octant and reaches the position No. 2, A will have run through its own proper octant, and 750 miles more, for which it finds room by running to that distance *outside* of its orbit; while, on the other hand, B saves a like distance by falling 750 miles within its orbit; so that all the three points have travelled precisely the same distance in the same time, each 180,000 miles. The points *a* and *z* now come into play, as the outer and inner points of circumference: when the mass completes its second octant, *a* is found, like A, to have run another 750 miles outside of its orbit, while *z* has fallen within its orbit to the same extent. The points *b* and *y* now come into play, and repeat the operation, and so on, through the remaining octants, so that when the revolution is completed, A has made up its deficiency of 6000 miles by revolving or rotating round C, whose external circumference, containing the points A and B, is exactly of that dimension; while, on the other hand, B has saved 6000 miles, by constantly falling back in the opposite direction; and the general result of the process is that the three points have travelled precisely the same number of miles; C along the mean orbit of the mass; but A and B along two new orbits, each of precisely the same dimensions as C's, but drawn round different centres, corresponding to their respective diametric distances from C. Their orbits are indicated in the diagram by the two dotted circles. In no other way could the feat have been accomplished, than by A and its successors *a*, *b*, *c*, &c., in succession, turning the mass round on its axis to the extent of one-eighth part of its circumference during its progress along each octant of the orbit, while B and its successors were simultaneously falling back to the same extent, in the opposite direction. Whatever be the size of the revolving body, and its proportion to the circumference of its orbit, the process and the result will be, in all cases, precisely the same. It may be scarcely necessary to add, that what takes place with the octant points, happens equally with every one of the body's intervening points, all of which, at every instant of progression, take precisely the same course as the octant points specially referred to. At every instant each of them takes a step forward, while the axis is turning in the opposite direction, without carrying them back to their original places, relatively to itself.

This great mistake of Galileo's, later astronomers and mechanicians have fully adopted; and, believing the moon to be fairly represented by the water in the basin, and that the water so placed does not rotate, they have very naturally indeed, but too hastily, jumped with him to the conclusion, that, to enable the moon to "*bring* the same point of her circumference continually *back* to the radius," she must have a power to rotate, and does rotate, in a direction "*contrary* to that of her revolution." "Contrary," the direction of this alleged rotation must be called, for unless it were "contrary" it could not be true; and yet, "contrary," as it is called, it is not the less precisely in the *same direction* as the line of revolution; for it is from west to east, and the revolution is so too. Galileo might have taught them better, for he rightly says, that the water's visible rotation in the basin is *contrary* to the direction of the basin's orbital revolution; and if that be correct, as it unquestionably is, then must the moon's alleged rotation in the opposite direction, from west to east, be not contrary to, but in the *same* direction as, the revolution; and, being in the same direction, its inevitable result, if it existed, would be the presentation of every part of her circumference in succession to the earth. As a wheel, moving along a rail, and rolling forward in the same direction, carries every part of its circumference in succession to the rail, and past a straight line drawn across it through its axle perpendicular to the rail, so would the moon, revolving as she does, and rotating, as alleged, in precisely the same way as the wheel, carry every part of her circumference past her radius-vector, and all round about. But, besides adopting Galileo's error, some of them have made a far greater mistake of their own, for they have supposed, what he did not, that there is a *third* possible way of an observer's seeing the whole circumference of a globe, namely, by having it carried round about him without turning round on its own axis, or any other point or pivot of revolution in his presence. Such a result, without rotation or turning round, is plainly impossible, and the object of all my arguments and experiments is to show that it is so. In this respect they have acted quite in the spirit of the Ptolomeans, who went on continually adding new cycles and epicycles, to explain the phenomena of the universe, till Copernicus displayed to their astonished eyes the simple truth, and demolished the complicated fabric that had been the work of many ingenious minds during many ages. The rotation which astronomers ascribe to the moon is just as purely hypothetical and groundless as any Ptolomean epicycle; yet has this doctrine been allowed to remain for almost two centuries a foul blot on the temple they have reared to the glory of physical science.

In the article ASTRONOMY, in the Encyclopædia Edinensis, (I. 510,) said to have been written by "George Buchanan, Esq., Civil Engineer, Edinburgh," the moon's rotation is thus described and demonstrated:—"From the motion of the solar spots has been inferred the rotation of the sun on his axis. The same conclusion is drawn in regard to the moon, from the apparent rest of her spots. For, as we go round and round a building when we wish to have a view of it on every side, in the same manner would the moon present to us, in succession, every portion of its surface, if, without moving on its axis, it only revolved round the earth, *the appearances being evidently the same as if the earth went round and round the moon at rest.* Thus, if a spectator on the earth should observe a remarkable spot, S, (fig. 8,) on the centre of the full moon at M, he would *evidently* lose sight of it by the time the moon, *if immovable on her axis*, arrived at her quarter at *m;* and another spot, *s*, which he did not at first observe, would now occupy the same centre position; and in order that the same spot as at first should still be observed on her centre, it would be absolutely necessary for her to make a quarter rotation in a direction contrary to that of her revolution. When the moon, therefore, in revolving round the earth, still presents to us a figure whose general appearance, though extremely irregular, is always the same, and which must on this account be undoubtedly the same half of her surface, we may be assured that *the moon has really a rotation on its axis*, in a direction *contrary* to and keeping exact pace with her revolution round the earth."

To the eye of common observers the sun and the moon *appear* to be both alike satellites of the earth, and, as such, to go round about the whole heavens in very nearly the same track. But, if the one of them, the sun, turn on his axis, and, by so turning, show a succession of spots flitting across his disk, should we not expect to see the same effect produced by the rotation of the other moving body, the moon, in circumstances so precisely similar? No, says Mr B.; for, though the flitting of the spots across the sun's disk is the effect, the sign, and the evidence of his rotation, and, consequently, were he not to rotate, these spots would stand still; yet, is the standing still of the spots on the moon's face the effect, the sign, and the evidence of her rotation also! But, on what principle he could found such an answer is anything but evident; and it is astonishing that the statement of his own argument did not make him doubt the truth of the received doctrine on this subject. To make bad worse he proceeds thus; "As we go round and round a building, when we wish to have a view of it on every side, in the same manner would the moon present to us in succession every portion of its surface, if, *without moving on its axis*, it only revolved round the earth, the appearances being *evidently the same*, as if the earth went round and round the moon at rest." Such is a specimen of the *reasoning* with which astronomers attempt to justify their paradox; and such reasoning has been deemed sufficient to overrule the irrefragable evidence of non-rotation, supplied by the fact of the moon's presenting the same face continually to the earth. So far, however, are the movements, which Mr B. compares, from being "evidently the same" in their results, that by a very simple proof we shall find them to be totally different. I cannot, indeed, conceive, and should like much to be told, *how* it is possible for every portion in succession, of the surface of any body whatever, whether revolving in an orbit, or stationary, to be seen by any observer, but in one of two ways: either by the observer's going round it, or by its turning round on its own axis, or some other point or pivot of revolution, in the observer's presence. This alternative affords the true opposites that Mr B. should have contrasted, and not the observer's going round the body, or the body's going round the observer, without "moving on its axis."

"If," proceeds Mr B., "a spectator on the earth should observe a remarkable spot, S (fig. 8), on the centre of the full moon at M, he would *evidently* lose sight of it by the time the moon, *if immovable on her axis*, arrived at her quarter at *m*; and another spot, *s*, which he did not at first observe, would now occupy the same centre position; and, in order that the same spot as at first should still be observed on her centre, it would be absolutely necessary for her *to make a quarter rotation in a direction contrary to that of her revolution*." So far, however, is all this from being "evident" and "necessary," that it could only happen in the case, which he tacitly assumes without reason or proof, of the moon's moving in her orbit perfectly free from the action of a centripetal force; or, in other words, if the centripetal force, or the earth's attraction of gravitation, were to act upon the moon's centre only, leaving her circumference, and all the other molecules of her mass between that and the centre, *free* to obey the impulse of the centrifugal or tangential force, which would, in that case, carry them all forward with the same velocity as the centre itself, and consequently all once round about that centre during the period of each orbital revolution. The changes, then, that would happen to his "remarkable spot S," would be really the effects, the signs, and the evidence of the moon's rotation, or turning round on her axis. Mr B., in common with other astronomers, has overlooked the very important difference that exists between the moon moving under the antagonist centrifugal and centripetal forces and an artificial body of the same form, a globe or a disk, which may be made to move in an orbit under the influence of the tangential force alone, perfectly free from any centripetal attraction. Such a body may be made to *shuffle* in a certain way round its orbit, presenting every point of its circumference to the centre of that orbit, *apparently* without rotating; but, even in that case, a real retro-rotation or backward-turning of the shuffling body on its own axis can easily be proved; and *without rotation*, either backwards or forward, the presentation of every point of its circumference to the centre of its orbit, or to a spectator within the periphery of the orbit, is perfectly impossible. On the other hand, were this artificial moving body to be subjected to a centripetal force, in the shape of a string, or other material vinculum, connecting any one point, or by two strings, or other material vincula, connecting any two diametrically opposite points of its circumference with the centre of its orbit, it would then, in every orbital revolution, necessarily and inevitably present, like the moon, the same face continually to that centre, without requiring any "quarter," or complete "rotation, in a direction contrary to that of its revolution."

Gravesande's illustration is this: "The effect of the moon's motion round her axis is, that she turns continually the same face to the earth. Suppose the moon in N (fig. 9); the face looking to the earth is *m n i*. *If* the moon were not to rotate on her axis, and her individual particles were to move forward in parallel lines, the line *m i* would coincide with the line *l n*, in the moon's position B, and the hemisphere *m n i* would coincide with *l m n*. But, seeing that while the moon describes the quarter of her orbit, she also accomplishes the fourth part of her rotation, the face which should coincide with *l m n* is in *m*, *n*, *i*; that is, again looking to the earth T. In the same manner is it proved that the same face *m n i* is seen by a spectator on the earth in the positions P and E." *Philosophiæ Newtonianæ In-*

stitutiones, &c. Leidæ et Amstelodami, 1728 : pars i. cap. 7, p. 385. The noted particle *if* never occupied a more important place than here. *Give* all that he requires, and the conclusion of his argument would be correct. Refuse, and it falls to the ground at once. The moon does not rotate on her axis, nor do her particles move in parallel lines; they move in concentric ellipses. Another illustration substantially the same as this, is to show that a body moving along the diagonal of a square or parallelogram, would reach the end of the diagonal without turning round on its axis. So it would most certainly; but the propounders of that illustration, as well as M. Gravesande, forget, or overlook the fact, that the moon or other body revolving in an orbit has a certain thickness, and that the molecules of its mass describe different orbits of different dimensions in proportion to their distance from the orbit's centre, and that at *every angle* of the rhombus or four squares, which they make use of to illustrate the moon's orbital revolution and rotation, the point of the circumference *a*, in one with the radius-vector at starting, *is carried* 90° *away from the radius*, and that at each of these angles the direction of the line of motion is also changed 90°; and that these four changes of 90° each, make up a complete rotation of *a* round the axis, during the orbital revolution. In fact, the only substantial difference between the case of revolution in a circle, and revolution along the four sides of a square or a rhombus, is that in the former case, the rotation is performed gradually and imperceptibly, and in the latter only four times, to the amount of 90° at once. It is true, that in the latter case the rotation is effected by means of the axis being four times *drawn back*, away from the advanced point *a;* but the result is not the less precisely the same; for round about the axis *a* proceeds, and this is proved irrefragably by its assuming every varying degree of angular distance in its progress away from and back again to, the radius-vector facing the centre of the square. If the body were to stop at any angle, or in the middle of any side of the square, and turn once round on its axis, from east to west, while the square was carried past it in the same direction, it would exhibit all the phenomena of *parallelism*, associated with unquestionable rotation.

In the article MOON, in the Penny Cyclopædia, the author proceeds in the usual manner. "The discovery of the telescope, and the examination of the moon which followed, soon shewed that the planet always turns the same face towards the earth, or very nearly. *From hence it immediately follows that the moon must revolve round an axis in the same time as that axis revolves round the earth.* If any one should walk round a circle, without turning himself round, that is, *keeping his face always in the same direction*, he would present alternately his front and back to the interior of the circle But, if he desires to turn his face always inwards, he must turn round in the same direction as he walks round." The logic here is the same as Mr Buchanan's, and is just a new version of the old fallacy, which I have already detected and exposed;—taking for granted, as an indubitable fact, what is nevertheless a physical impossibility,—the presenting of "every part of its circumference in succession" to the centre of its orbit, by a globe revolving in an orbit, *without rotating on its axis.* Our walking gentleman himself shall prove this. Let him be tied to the centre of his circle, as the moon is to hers by her centripetal radius of gravitation, and then let him walk round the circle, *if he can,* with his face constantly pointed in the same direction to a distant object outside. He will find the latter condition impossible unless he turn backward on his own axis, simultaneously with his forward orbital movement. Suppose him to be tied to his circle by a cord fixed round his waist. As he moves round his orbit he will find that the noose will make him keep the *same side,* or *face,* or *back,* as the case may be, directed constantly to the centre of the circle, unless he draw himself continually away from its knot and the radius-vector by moving "left shoulders forward." One of two things seems perfectly certain: either the noose and its knot revolve about the man, or the man turns backward on his own axis within the noose; but the knot being fixed to the radius, and the radius to the centre of the circle, it is quite obvious that they cannot revolve about the man, therefore it must be the man that retro-rotates within the noose. This has been abundantly shown already, and is just another version of the water in the basin. Let the walking gentleman turn round again on his own axis, in the direction of "right shoulders forward," within his noose, while going round his circle, and he will find his face and back and two sides, in turn, or, in other words, "every part of his circumference in succession," presented to the centre of the circle, thereby completely disproving the doctrine he was brought to prove, namely, that the moon's presenting the same hemisphere continually to the earth is the effect of her rotation on her own axis, from west to east, the same direction which the experimenter had last taken, in his turning round. The author of this cyclopædiac article has just assumed that the same effect as would be produced by a person's moving first straight forward along a given line, and then moving straight backward to his starting point, would be produced were he obliged to go out of his straight path round some intervening obstacle, such as a round table placed upon the line. But, in the latter case, he would be obliged, in going from north to south, for instance, to shuffle out of his way to the left, and, in returning, to shuffle again to the right, in order to get round about the obstacle. In making these shuffles, the opposite points of his own circumference *cross each other's paths;* because at every instant of progression the central axis of his body changes the direction of its line of motion, without the points of his circumference turning back in the same proportion, so as to occupy always their original places on the same side of the axis. This certainly does not happen when he moves straight forward and backward; and these crossings of the different orbits described by the different points of his own circumference are equivalent to, and precisely effect, one complete rotation on his own axis. All the parts of any horizontal section of his body move with the same velocity; and, to enable them to move through equal spaces in equal times, they must find room for their movement by some going to the outside, while others are falling back to the inside, of the orbit described by his perpendicular central axis; and the reason why he keeps his face always in the same direction, while he is really rotating, is that what his circumference gains by rotation in one direction is lost by his centre's moving forward in the opposite direction. Common sense might tell him this; but the string and the noose round his body confirm its dictates undeniably. The same would be the result of his shuffling along the four sides successively of a square, or a parallelogram, or a rhombus, with his face looking invariably in the same direction; north, or south, or east, or west, as the case might be. At each of the four corners, all the points of his body cross each other's paths horizontally: and, as before, these horizontal crossings amount to one complete rotation round his perpendicular axis. To make the experiment more complete, let him tie himself to the centre of his circle by a slack cord, simply attached to a button or button-hole of his vest, without a noose, and then shuffle round, with his face always looking in the same direction. When he completes his revolution he will find the cord drawn round about him. How could it get into such a position unless he had turned himself round on his own axis, and drawn it round along with that point of his outward man to which it was attached? It has not crept round about him of itself like a living serpent; yet has it been stretched out to the extent of a whole circumference of the moving body; a fact that, I presume, can only be accounted for by that body's having turned round on its own axis, and drawn the cord round about with it. Let him now, for a final experiment, just stop at any point of the circle, with his face to the centre, and the cord attached, as before, to his button or button-hole, and turn himself once round on his own axis, from east to west, or with his right shoulder backwards. The cord will be drawn round about him, and this result will be admitted to be the effect of his retro-rotation. But, when precisely the *same effect* is produced gradually during an orbital revolution, what is so likely to produce it as the *same cause*, the revolving body's retro-rotation on its axis, in a direction *contrary* to that of the revolution performed by the axis itself?

The Marquis de la Place, in his "System of the World," translated by J. Pond, F. R. S, (the late Astronomer Royal,) Book i. ch. iv. p. 59—62, says, "The lunar disk contains a great number of invariable spots which have been observed and accurately described. They shew us that this body always presents to us very nearly the same hemisphere; it turns then upon itself in a period equal to its rotation round the earth, for if we imagine an observer placed at the centre of the moon, supposed transparent, he will see the earth and its visual ray revolve about him, and since this ray always intersects nearly the same point of the lunar surface, it is evident that this point must revolve round the spectator, in the same time, and in the same direction as the earth. Nevertheless, continued observation of the lunar disk has discovered some small diversity in these appearances; the spots are perceived to approach to and recede alternately from the limb; those that are very near the extremity appear and disappear successively by periodical oscillations, which have been distinguished by the name of the *libration of the* moon. To form a just idea of the principal causes of this phenomenon, we should consider that the disk of the moon, seen from the centre of the earth, is terminated by a great circle of the lunar globe, perpendicular to the radius drawn from this centre to that of the globe. It is upon the plane of this great circle that the hemisphere of the moon is projected, directed towards the earth, and its appearances arise from its *motion of rotation* relatively to its radius-vector: if it was without a motion of rotation this radius-vector would trace at every lunar revolution, the circumference of a great circle upon its surface, every point of which would be successively turned to us; but at the same time that the radius-vector traces this circumference, the lunar globe by its revolution brings always very nearly the same point of its surface to this radius, and consequently the same hemisphere towards the earth. The inequalities of the motion of the moon produce some small variations in the appearances, for the motion of rotation not partaking in a sensible manner of these inequalities, it is variable with respect to its radius-vector, which thus intersects its surface at different points. The lunar globe, therefore, makes, with respect to this radius, oscillations corresponding to the inequalities of its motion, which causes some part of its surface to be alternately concealed, and exposed to our observation. All these causes produce only an apparent libration of the lunar globe;

they are mere optical illusions, and do not affect its real motion of rotation."

It would scarcely be credible that this should have proceeded from such a man as La Place, were it not in perfect consistency with the prevalent opinions of astronomers on the subject of the moon's rotation. "An observer," he says, "placed at the centre of the moon, supposed transparent, *will see the earth and its visual ray revolve about him.*" The meaning of this passage is not very obvious; but probably it is this: The observer must be perfectly separate from, and independent of, the moon's mass, and capable of revolving about the earth unaffected by the moon's motion. In this case, says La Place, he might revolve without rotating, while the moon does rotate in the direction alleged, from west to east: and the result would be, that the same point of the moon's circumference, with the radius-vector and the earth's visual ray, or the earth herself, would appear successively on every side of the observer, or, as La Place says, would revolve about him. But the observer's apparent standing still in these circumstances is nothing else than that retro-rotation from east to west which has been mentioned so often; and the fact of his retro-rotation might be proved by attaching the observer by a cord to that point of the moon's circumference that is always in one with the radius. At the completion of his revolution the cord would be drawn round about him; and this could not have been occasioned by the radius revolving about him, for that is physically impossible. He will be placed in circumstances precisely similar to those of the walking gentleman, whose case we have so recently considered; and either gentleman might be very well represented by a thimble or a cork placed on the axis of the magnetic card of a marine compass carried round a circle. As the box revolves, with the same point of its circumference continually directed to the centre of the orbit, that point, with the centre itself and the radius-vector, will undoubtedly appear successively on every side of the gentleman's representative; not, however, because they revolve about him, which is impossible, but because he is continually turning himself round in a direction contrary to that which is taken by his own axis and the box that contains it, in performing their orbital revolution.

La Place adds, "since the earth's visual ray always intersects nearly the same point of the lunar surface, it is evident that this point must revolve round the spectator, in the same time, and in the same direction as the earth." So here we have a broad assertion of the proposition, that not only that point of the moon's circumference which remains in one with the radius-vector, but also the radius itself, and the earth which it repre-ents, *actually revolve round about the moon's centre.* Such a revolution is just as impossible as it would be for a cannon ball to fly from earth to heaven, and again from heaven to earth and round about the stars, *without moving from the gun.* The earth never quits its position in, or near to, the centre of the moon's orbit; the radius-vector merely represents a certain point of the earth's circumference considered as expanded to the size of the orbit; and a certain point of the moon's circumference remains continually attached to the radius; how, then, is it possible, in the nature of things, that they can revolve round about a body, or point, or person that not only remains, as the moon's centre always certainly does, at a distance from, and to the outside of, the circle described by her inner point of circumference; but is also continually in motion round about the very objects—the earth, the radius, and the moon's attached point, that are said to revolve about it? The earth revolves about the moon's centre, but *without leaving its fixed place, in the centre of the moon's orbit,* at the very time the moon's centre is revolving about the earth. This is just the rod AB in one of its worst phases; and the very error committed by Galileo, when he thought he must have gone round about the water *if,* as he inferred, the water had not turned round on its axis. The earth, considered relatively to the moon, moves not at all; the nearest point of the moon, in one with the radius-vector, describes an orbit of 1,434,000 miles; while her centre, a thousand miles farther from the earth, describes an orbit 6000 miles larger, yet do both the earth, which never moves, and the moon's point which describes the smaller orbit, perform a revolution *round about the point which describes* the larger orbit, and which consequently remains invariably on the outside of them. How the earth, represented by the point A, and the radius-vector, represented by the body of the rod AB, could revolve about the moon's centre C, without the earth forsaking her fixed central position and crossing the moon's orbit twice, in going to the outside and returning, in the course of her circumgyration, far transcends my comprehension; and, were it not that such men as Galileo and Laplace have said it, I should scarcely have believed it possible for any human being to deliberately sanction and promulgate an opinion that seems so preposterous.

Laplace procceds to say: "If the moon were without a motion of rotation, her radius-vector would trace, at every lunar revolution, the circumference of a great circle upon her surface, every point of which would be successively turned to us." I have shown already that this is impossible, and need not repeat my arguments and illustrations here. But, either he or his translator "brings" the absurdity to a climax when he says: "At the same time that the radius-vector traces this circumference, the lunar globe by its revolution brings always very nearly the same point of its surface to this radius." So this great mechanician seems to have believed it possible for a radius-vector that remains always positively attached to the same point of a globe, to describe a circle all round the globe's circumference, without ever leaving or stirring from the point to which it is so attached. For "at the same time that the radius traces the circumference," the globe itself, moving apparently in the same direction, "brings always very nearly the same point of its surface to this radius." This means in plain English, that the radius *never* describes any circle at all round the moon's circumference, *because* the same point of circumference remains always attached to it, and will not allow it to set out on its circumferential journey, which in consequence is never made.

Far be it from me to think of disparaging the fair fame of Laplace; but, in this case, he seems to have been very greatly mistaken. Whether, however, the mistake lie with him or with me, and, whether or not I have made out the point I have been contending for, I shall now leave it to the public to determine, and the determination may be made very easily, for the demonstration is grounded on the following simple questions: 1. Is it possible for a ball, fixed on the point of a rod or stick, to turn round on its axis while it remains so fixed? 2. Is it possible for a wheel moving along a rail to turn round on its own axis, and at the same time keep the same point of its circmuference continually in contact with the rail? 3. Is it possible for a rod or stick to turn round on both of its two ends at once? or to turn round on its middle while one end is held fast? Is it possible for a cord, attached by one end to the centre of a circle, and having its other end attached to the surface of a globe or wheel, either revolving round the circle, or remaining stationary at one point, to be drawn round about the circumference of the globe or wheel, unless that body turn round on its axis? To these questions I answer No; but the astronomical doctrine I have been combating requires all the answers to be in the affirmative. Which of us is right?

J. L.

Edinburgh, 18*th March* 1847.

CORRIGENDUM.

P. 11, line 34, *after* degree, *insert*—"of its circumference, while moving through one degree."

Fig. 1

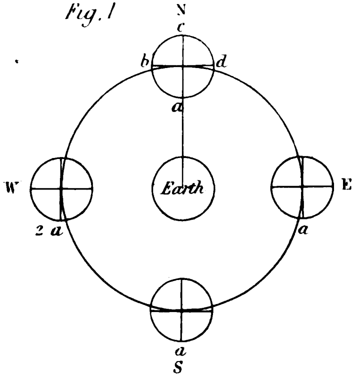


Fig. 2

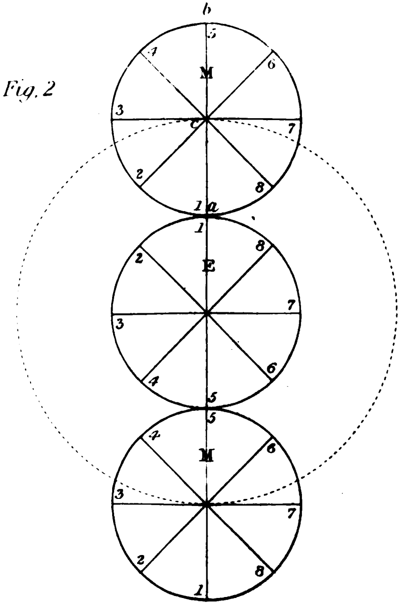


Fig. 3

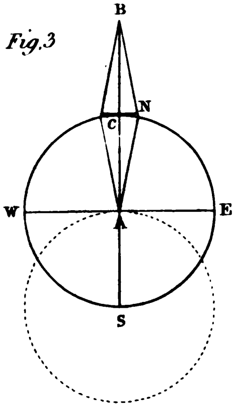


Fig. 4

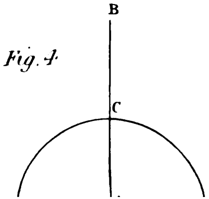


Fig. 7

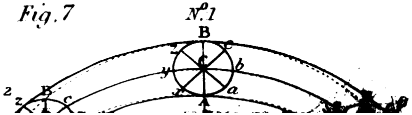
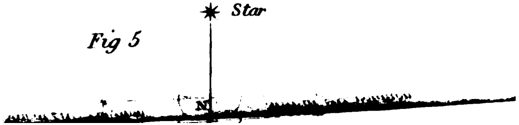
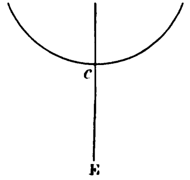
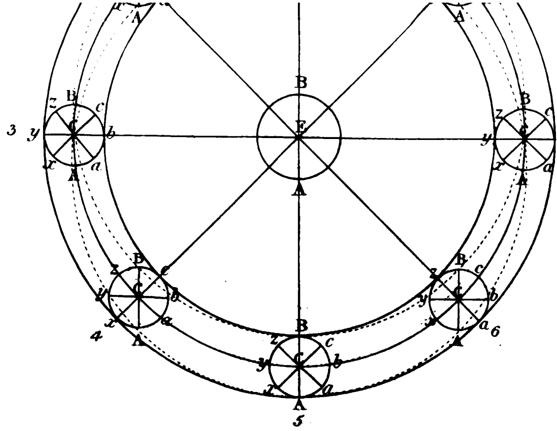
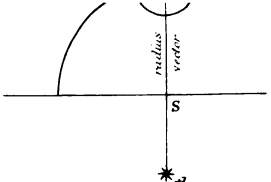


Fig 5









Star at 180th. rotation

Fig. 6

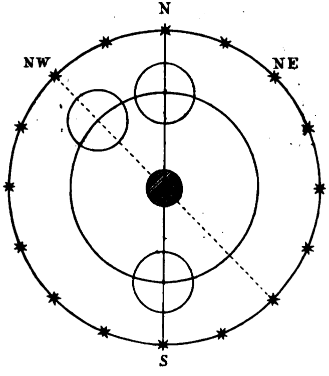


Fig. 9

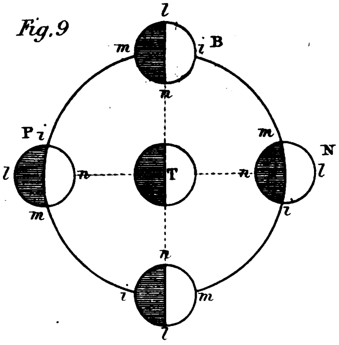
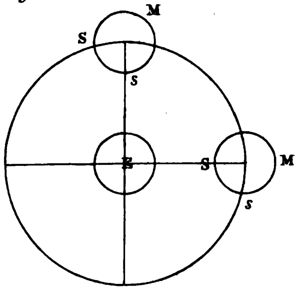


Fig. 8



Geo. Aikman Sc